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THE

ANTIDOTAL TREATMENT

OF DISEASE.

BY JOHN PARKIN, M.D.,

CORRESPONDING FELLOW OF THE ROYAL ACADEMIES OF MEDICINE AND
SURGERY, OF MADRID, OF BARCELONA, AND OF CADIZ; FELLOW OF
THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH; OF THE
ROYAL COLLEGE OF SURGEONS, LONDON; OF THE ROYAL
MED. CHIR. SOCIETY, LONDON, ETC., ETC.

PART I.

"*The bane and antidote are both before ye.*"—SHAKESPEARE.

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INTRODUCTION.

The History of Medicine.—Of all sciences, the science of medicine is, perhaps, the most ancient. As diseases must always have existed, the necessity of discovering a cure for them, as Sydenham has remarked, would have induced mankind to study medicine long before the birth of the Grecian, or, even, of the Egyptian Esculapius, who flourished a thousand years before the former. That medicine was cultivated among the Egyptians, with great zeal and ardour, and from a very remote period, would appear certain; kings themselves, according to Pliny, assisting at the opening of dead bodies, in order to ascertain the causes of disease. But, although we are certain, that the art of healing was thus held in such high esteem, we know nothing of the mode of treatment. We only know that the Priests were the Physicians, and that Tablets were erected in the Temples, on which were inscribed the cures effected, and, also, the means employed. These were afterwards collected into a book called *Embre*, *Scientia causalitatis*, and to this code the Physicians were obliged to conform. That there were Physicians in Egypt, we learn from Holy Writ; for it is stated, "Joseph commanded the Physicians to embalm his father; and the Physicians embalmed Israel." As this history is referred, by Chronologists, to 1,660 years before the advent of Christ, it would be about 100 years after the foundation of Greece. Hence we may infer, as is generally the case, that the Greeks derived their knowledge of medicine from the Egyptians.

Independently of the Egyptians, the art of healing was practised among the Hindoos from a very early period; as far back, in fact, as their history extends. With them, also, the Bramins, who cultivated all the other sciences, were the first physicians; but one particular sect—the Samaniens—devoted themselves entirely to the study and the practice of medicine, considering that the study of the other sciences would be injurious.* With this nation, the same as with the Egyptians, the science of medicine, like the other sciences, has been long since lost; but they are now learning it again from a race, that was in a state of barbarism when they were skilled in the art!

We may now turn to the Greeks, who, although not the founders of the Medical Art, are the people to whom the moderns are indebted for their knowledge in medicine. Theirs, in fact, are the only complete works, that have descended to us, on this almost Divine Art.[*] Esculapius is generally regarded as the father of medicine; but Apollo, who practised this art, in common with the mysteries of the Temple, was so considered by the Greeks. After his death, he was elevated into the rank of a god, and worshipped as the God of Medicine; while Diana, his sister, became the Goddess of Medicine. The priests therefore in Greece, as well as elsewhere, became the first physicians. At a later period, temples of health, named Asclepia, were erected in various parts of Greece, being presided over by an order of Priest-Physicians, called *Asclepiadæ*, actually priests of Esculapius—*Ασκληπιος*. These Temples were situated on high and healthy spots; sometimes, according to Sprengel, near to mineral springs. They were hospitals, or receptacles, for the sick; the patient being subjected to a sort of purification on his entrance, and, afterwards, to a regular course of bathing, frictions, fomentations, etc.—all powerful agents in the cure of many diseases, more particularly chronic ones. Their effect would be increased by the healthiness of the site: while something, perhaps, may be ascribed to mental causes—the imagination of the patient being excited by the rites and ceremonies enjoined. As the cure of diseases was ascribed to the favor of the Gods; their occurrence was considered to be caused by their anger. Hence the necessity of oblations and sacrifices, as well as prayers; in answer to which, the patients expected the means of cure to be revealed to them by dreams. After a time, the Priests, taking advantage of the credulity of mankind, as the Priests of all false religions have invariably done, pretended that the Gods revealed their secrets only to them. They thus became the Oracles of the Gods and the Divinities of men, to whom all must look for salvation and health. There has been the same tendency even in the Christian religion, from almost its first foundation to the present day, to exalt the Minister to a level with God —the creature above the Creator; the servant above his Master—and to induce weak, credulous, men to seek salvation at his hands, or through his means, instead of at the only fount where it is to be found. It is the spirit of Sacerdotalism, which has ruined more souls than the Spirit of the Gospel has saved.

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* There is a Hindoo proverb, which says:—"Learning hath small skill in surgery," an axiom that will also apply to the practice of medicine, as we shall presently find.

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[*] Hippocrates says :—*Ιαπρος φιλόσοφος ισω θεος.* "Medicus Philosophus Deo par."

Up to the fifth Olympiad, Medicine was practised exclusively by the Priests; but, at this period, certain Philosophers began to encroach on their prerogatives. At first, they made use of superstitious rites, like the Priests; but they soon threw off the mask, and proclaimed, that they cured diseases by natural, not supernatural means. In this way, the foundation was laid of that Science, which has continued, with varied results, up to the present time—more than 23 centuries. It is not, however, until the time of Hippocrates, that we have any clear and accurate account of the practice of Medicine among the Ancients: for his are the first works that have descended to us, in a complete state. As Professions in Greece, at that time, were hereditary—as they still are in China, as well as trades and all occupations—there were, in the space of 200 years, seven Physicians of this name. One of them enjoyed a great reputation in the time of Solon, about 580 years B. C. They have left 72 works in all, but the majority are ascribed to the son of Heraclides, called the great Hippocrates, who was born in the 80th Olympiad, 460 years before the Christian era. It will be interesting, if not important, to ascertain the opinions of him whom the Moderns regard as the Father of the. Medical Art—the *Romulus Medicorum*, to use the expression of Sydenham. "Strong in the knowledge of his contemporaries and predecessors, he collected their different opinions, and was the first to form an imposing system, which gave a rank to medicine and a separate existence."* He separated the study of Medicine from that of Philosophy: that is to say, he banished from medicine the subtleties of the philosophic sects; the application of their imperfect physical science; and the hypotheses by which they attempted to explain all the phenomena of nature, even before they had observed or examined them. He showed that observation is, in medicine, the same as in other sciences, the only road which leads to the knowledge of those truths, that the human mind is enabled to discover. This laid the foundation of the experimental system.

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* Dictionnaire de Médicine.

According to Hippocrates, health is the result of the proper admixture of the humours of the body, which he called κρασις—mixture. To this he added another doctrine, that of *Coction*, which, aided by the heat of the body, converted these humours into other substances, in order to form a homogeneous compound. He accounted for diseases in the same way. As health was produced by a proper mixture of the humours; so, disease was due to an improper, or faulty, admixture.[*] As a consequence of this, or, from an unnatural, instead of a natural, amount of heat, the humours underwent some change—a concoction or despumation[†]—and were converted into a morbid, instead of a healthy, fluid or substance. This morbid matter, according to the hippocratic doctrine, was the real, exciting, cause of disease—the *materies morbi;* a term that has been employed by writers from that day to the present. But, although it was by means of coction, that this morbid matter was formed, the continuation of the process brought the matter to maturity, and thus changed its quality from acrid to mild. Thus, in influenza, or affections of the mucous membrane of the air passages, the secretion is at first thin and acrid: but, afterwards, becomes thick and mild—it is a sign of amelioration. All those diseases in which this change was not observed—as in cancer—were considered incurable. It was also inferred, that this morbid matter was eliminated from the system by the different secretions and excretions of the body, either gradually or by some sudden and violent effort. This was termed the crisis, or termination of the disease—properly, expulsion of the morbid matter from the body.

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[*] These humours were four, viz., the blood, the phlegm, the bile, and the atrabile.

[†] The concoction or despumation—*de* and *spuma,* to take the froth off—of the febrile matter, signifies nothing more than the separation of the morbific particles from the sound ones.

Another mode of termination of diseases, according to Hippocrates, is by what he termed *deposit* —*αποστασις*. This occurred when the morbid matter, instead of being expelled, was thrown back upon some part of the body and deposited there—producing swelling of the joints, inflammation, or gangrene. It is what we now understand by metastasis: an effect frequently observed in Gout.

Great attention was paid by Hippocrates and his disciples to the Prognosis of every disease; in order to ascertain its nature, its probable duration, and the manner of its termination. Little or no attention was paid by them to the names or the classification of diseases—a point considered to be of much importance by the moderns; they only regarded the symptoms, as a guide to the treatment; and in order to ascertain the probable issue of the case.

The treatment of Hippocrates may be readily understood from the doctrine he entertained. This was to favor the coction of the morbid matter, and to aid in its expulsion. We know little or nothing of the remedies he employed; but, as this wise Physician trusted much to the efforts of Nature, they would appear to have been of the most simple kind. As Dr. Barker justly remarks: Hippocrates never attempted to *cure a Fever*, according to the common acceptation of that word, i.e. *to put a stop to the febrile motions*, or to extinguish the Fever, by the rules of Art. His principal object was to assist Nature in her efforts for the expulsion of the morbid matter: correct her when wrong, but not to interfere with her operations, when they were properly directed.* To promote the Concoction, and Evacuation of the Febrile matter, he made use of the *Mulsum*, or Hydromel: warm fomentations and warm Bathing at the commencement, and at the height, of the *Crisis*. Although Nature cures diseases by *Hæmorrhage; Vomiting; Expectoration; Purging; Sweat; Urine* and *Abscess*, Hippocrates never employed bleeding or vomiting to bring on a Crisis—both these being considered dangerous at such a time. The crises, which he endeavoured to imitate by Art, were only those of *Expectoration*, *Perspiration*, and *Purging*. Although Hippocrates endeavoured to follow Nature's method in the cure of diseases: that is to say, to produce diaphoresis in Fevers, to administer evacuants in bowel complaints and so forth, he sometimes adopted the opposite mode—the treatment by revulsion. Thus, he gave purgatives in some cases of obstinate vomiting: and he administered evacuants—both emetics and purgatives—in certain fevers, as the moderns are in the habit of doing. He did not, however, employ this method generally, as was done subsequently by certain sects, who adopted the rule—*contraria contrariis opponenda.*

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* Hippocrates defines nature to be the aggregate of all things that concur to perfect health.

Such were the doctrines and the practice of the Father of the Medical Art; and although we may sometimes smile at his opinions, respecting the immediate cause of disease—the mixture and coction of the different humours—the doctrine of a peccant humour, or morbid substance in the fluids, has been received and rejected; adopted and repudiated, at different periods, from that day to the present. But whether received or rejected, the treatment based on the hippocratic doctrine is the only successful method, as we shall hereafter find, that has been adopted up to the present time.

Notwithstanding that Hippocrates had separated the study of Medicine from that of Philosophy, they again became united after a certain interval. Hence a new Sect was formed, that of the Dogmatists. Instead of taking experience and observation for their guide, and directing their attention to the treatment of diseases; they were engaged in discussions respecting their secret and occult causes. The science of Medicine, therefore, became the arena for all kinds of contradictory opinions; and the treatment of disease was the prey of the most lamentable chance.

Struck by the vanity, the variety, and the contradictions, of the prevalent theories; and of the absurdity of the modes of treatment adopted in consequence, certain Physicians attempted to bring back the practice of medicine into the path it had pursued before. Like the sect of the sceptics, who submitted all philosophical opinions to the most rigid test, they rejected all opinions and theories not capable of proof, and based their treatment of disease on experience alone. The search after causes, which do not fall within the sphere of the senses, they considered to be not only useless, but detrimental. This was the origin of the Sect of Empirics, which continued for six centuries, and until nearly the time of Galen. After this, the Empirics degenerated; no longer taking experience as their guide, they prescribed remedies without rule, and without judgment. Instead of continuing to be the followers and disciples of Hippocrates, they became Apostates to his doctrine, and the founders of the modern sect of Empirics.

Such was the state of medicine in Greece up to the commencement of the Christian era; at which period the art was almost unknown in Rome. For nearly 600 years, the Romans were without Physicians; but after the conquest of Greece, Archagatus, a Greek Physician, arrived in Rome from the Peloponnesus. This was in the time of Cato, 535 years after the foundation of the City, and 219 before the Christian era. Instead of favour and gratitude, Archagatus met with nothing but obloquy and ridicule, being nick-named the executioner, in consequence of his employing the cautery and the knife. He therefore returned to Greece, and left the inhabitants of Rome to the care of their Greek slaves; who, possessing some knowledge of medicine, were accustomed to attend upon them in illness. This continued until the time of Asclepiades, who may be considered the Roman Esculapius, not the Roman Hippocrates, for his doctrine differed materially from the latter. He formed, in fact, what has been termed the Atomic theory. Adopting the ideas of Epicurus, and some other Philosophers, he concluded that the body is composed of the union of Atoms, which affect certain forms. The regular movement and right proportion of the Atoms constituted with him a state of health; their irregular movement and disproportion, the state of disease; caused, as he concluded, by obstruction in the pores of the body, which these mis-shapen Atoms were unable to penetrate or pass through.* As a consequence of this theory, Asclepiades rejected the majority of remedies, that had been employed up to this period, and trusted to the efforts of nature, and to diet, for the cure of diseases.

Themison, one of the immediate successors of Asclepiades, although adopting the theory of the latter to a certain extent, made so many changes and additions, that it may be looked upon as a new and distinct one. He inferred, that the pores of the skin were sometimes relaxed, sometimes constricted; thus allowing of the passage of certain substances out of the body in the one case, and preventing their exit in the other. To these conditions —τονος ατονια, *strictum*, *laxum*—he referred nearly all the phenomena of disease. Hence, all diseases were placed by him in two groups, each of which required a different, or opposite, mode of cure—relaxants or calmants in the one case; tonics or stimulants in the other. He admitted, however, that some diseases were of a mixed character, and that they required a mixed mode of treatment. A modification of this doctrine has been revived in modern times, while the practice founded upon it has been continued up to the present time. Celsus, who flourished under Augustus in the 1st century of the Christian æra, and whose work is now studied, as a model of purity and elegance of diction, adopted the theory of Asclepiades, with a mixture of humoral pathology. As it is doubtful whether he practised medicine, he must be regarded rather as an historian than as a Physician—the narrator of the opinion of others rather than of his own.

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* This doctrine was revived some centuries later, as will hereafter appear.

It was under such circumstances that Galen appeared, in the middle of the second century of the Christian æra. Skilled in all the learning of the philosophical and of the medical schools: endowed with a vast understanding, and with all the qualities of a profound observer; Galen endeavoured to snatch the science of medicine from the state of anarchy in which it then was. But, he made no attempt to found a new Sect: to proclaim a new doctrine, or to establish a new mode of treatment. With a modesty and a nobleness of mind, that always accompanies true Genius, he avowed himself to be the Disciple of Hippocrates—the Restorer of his doctrine and of his practice. As a matter of course, he succeeded in his endeavours, and the Hippocratic doctrine again became dominant, although under a new name — that of Galenism.

"During 14 centuries, Galen was the God of Medicine; men affirmed only on his dictum. His authothority, like that of Aristotle, whose fortune he shared, as well as his Philosophy, was absolute; and it required long and unheard of efforts to overturn it."* This result is to be ascribed, not to the adoption and promulgation of other doctrines, but to the darkness—both spiritual and intellectual—that overspread the whole of Europe during the middle ages. Like all the other sciences, the Science of Medicine became obscured and degraded, if not extinct; there were no regular Physicians, and the practice fell into the hands of Magicians and of Quacks, and, ultimately, into that of the Monks and the Friars, as they thought it increased their credit with the people. "Some Monks," remarks one writer, "a little less ignorant, but not less superstitious than the half savage people by whom they were surrounded, took possession of the healing Art, if this name can be given to the rude empiricism which they adopted. In their hands, the art of healing became what it was in the first ages of the world. Instead of drugs, it was to prayers, to the relics of saints, and to sprinklings of holy water, that they had recourse to restore health."† "At this period," observes another authority, "medicine was confined to the Cloisters, and the medical monks did not differ from the Priests of Esculapius, excepting by their greater idleness, their ignorance, and the grossness of their prejudices, as also of their lives."* But they confined their practice—if such it can be called—to medicine: they repudiated that of Surgery. The Council of Tours (1163) prohibited the Clergy from performing operations, under the vain pretext, that the Church abhorred the effusion of blood—*Ecclesia abhorret a sanguine.* That this is true as regards the Church of Christ there can be no doubt; but *the Church of Rome*, after the massacre of St. Bartholomew, when the streets of Paris ran *with the blood* of the Huguenots, ordered a *Te Deum* to be sung, and a medal to be struck, to commemorate the event! It was also by the express orders of the Church of Rome, that, according to Terizonius, a million of human beings —the Albigenses—were massacred by the armies of France; while millions more have been sacrificed by her, in order to put down, what she terms "heresy"—*i.e.*, Christianity—through that "bloody" tribunal, the Inquisition. This is, indeed, straining at a Knat, and swallowing a Camel. It was, then, to the Church of Rome, that we are indebted for the unfortunate divorce of medicine and surgery, which, like man and wife, form one and the same body, and ought never to have been separated. Indeed, as the late Sir Richard Dobson quaintly observed, in his examination before a Committee of the House of Commons, respecting the functions of Physician of the Fleet: "a Physician is a Surgeon with his hands tied behind his back."! This was actually verified by a late renowned Physician, who allowed a medical friend, travelling with him, to die of an attack of apoplexy, while they sent to a neighbouring town for a Surgeon to bleed him!!

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* Dictionnaire de Médicine, vol. 19, p. 264. † Id., p. 270.

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* Diction. des Sciences Médicales, v. 32, p. 29.

From this state of degradation, the Science of Medicine was fortunately extricated by an accidental circumstance, the last that we should have expected would have been productive of such a result. This was the conquest of Spain by the Moors. Although the Arts and Sciences were proscribed by Mahomet; and although his followers were, at first, faithful to his injunctions, they gradually became cultivators of the whole, having acquired them from the nations they had conquered. In the 7th century, a work called the Pandectes of Medicine, written by Ahrun, a christian priest of Alexandria, and composed of extracts from Greek authors, was translated into Arabic; and complete translations of the Greek writers were published between this and the 9th century. To this source, the Moors were indebted for their knowledge of medicine. But, what is still more singular, they had also made considerable progress in Chemistry, as well as in Medicine, at this date. In the 8th century, Geber, of Mesopotamia, prepared, if he did not discover, Corrosive Sublimate, as, also, Nitric acid; Nitre; Muriatic acid; Red precipitate and Blue Stone. It is, in fact, to them that we are indebted for our knowledge of the elements of Pharmacy and of Chemistry. They also established a Medical School at Bagdad, consisting of a College, Hospital, and Pharmacy. After the conquest of Spain, in the 8th century, the Moors established a medical school at Cordova, and deposited there the original medical works of the Greek Authors, as, also, all the other works that had been saved from the destruction of the Library at Alexandria. When this fanatical act was perpetrated, the greater part of the Greek medical works were saved, by the intervention of Mahomet himself, it is supposed. It has also been stated, that the Library at Cordova contained 300,000 Books, while, in the 12th century, 70 Libraries had been established in that part of the Peninsula occupied by the Moors. They had also established Medical Schools at Seville, Toledo, Saragossa and Murcia. These schools soon attracted French students, who studied and translated the Greek medical and other authors; the result being, not only the revival of learning in Europe, but the establishment of the medical schools of Montpellier and Paris in the 12th century. The Spanish schools, after the expulsion of the Moors from Spain, were either suppressed or gradually became extinct. But, "the period when, by an effort of courage, the Spaniards recovered their independence and their liberty (by the expulsion of the Moors), ought to have been a sign of the progress of knowledge: it was the precursor of a state of ignorance, which each age has appeared to render more profound. It would not be difficult to point out the cause of this apparent anomaly. This task, which is foreign to our subject, would, were we to fulfil it, teach nothing to those enlightened individuals who understand the direful influence, which the establishment of the Inquisition must have exercised on the people of the Peninsula."* Since its suppression, and, with it, the absolute power of the Priests—the sworn enemies to all intellectual progress—Medicine and Surgery have made rapid advances; and there are, at this moment, Professors in Spain who may be favourably compared, for erudition and skill, with those of any other country. A regular divorce was thus again effected between Theology and Medicine, while the latter became, once more, elevated into a Science, to which the discovery of the art of printing lent no small aid. The works of Hippocrates, of Galen, and of other writers, were printed, and soon became the Text books in Medicine: while the doctrines and the practice of the Fathers of the medical Art reigned supreme. This continued until the commencement of the 16th century, when the humoral pathology, as it has been termed, became, once more, neglected and obscured, not from ignorance, and the want of knowledge, but, strange to say, by the advance of other sciences; or, by that of Chemistry.

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* Dict. des Sciences Médicales. Art. Médicine.

Basil Valentin had already inferred, that salt, sulphur, and mercury, were the primary elements of all bodies, when Paracelsus attempted to found a medical theory on these assumptions. He concluded, that there was either an effervescence of the salts; a combustion of the sulphur, or a coagulation of the mercury in particular diseases. When the salts predominate, atony and asthenic diseases follow: while fever, he considered, was produced by an excess of sulphur: mania, to a sublimation of mercury, and paralysis and melancholy to its distillation. Absurd as this doctrine is, it was, nevertheless, adopted to a considerable extent: and two societies were formed at the commencement of the 17th century—the Rosicrucians and the Rosians—with the object of propagating the opinions of Paracelsus. Willis, at a later period, advocated the same theory with some modification; having inferred that each humour, into which these elements entered, was converted into a ferment.

Such was the state of the Medical Art, when a new claimant arose to dispute the authority of the preceding by advancing a different theory. This was Van Helmont. According to him, each individual possessed a principle which he called Archæus, and which, distinct from the soul, presided over all the phenomena of life. When the Archæus was deranged,—the seat of which he considered to be in the stomach—the result was a ferment, which, being conveyed to other parts of the body, produced disease. Such being the theory, the principal object that Van Helmont had in view, in the cure of diseases, was to regulate the movements of this peculiar principle—to calm it when excited, and stimulate it when depressed. Hence he rejected all other remedies, and trusted to calmants and stimulants only.

At this period, or in 1619, an event occurred, the most memorable, and the most important, of any in the history of medicine—the discovery of the circulation of the blood. Important, however, as this discovery was, it produced no immediate result as regards the theory and the treatment of disease. In the middle of the 17th century, when Harvey's discovery had become generally acknowledged and admitted—for it took nearly half a century to establish a truth, that admitted of immediate and ocular demonstration—Silvius, a Professor at Leyden, proposed another chemical theory. Instead of referring disease to the effervescence of the salts, sulphur, etc., in the body, he ascribed it to the acrimony of the humours—which were either acid or alkaline—and treated it accordingly. Proposed by an eloquent and distinguished Physician, this theory, a simple and plausible one, was extensively adopted at the time. Stahl however combated all chemical theories, and ridiculed the idea of explaining, by the laws of inorganic matter, the functions of the living body. He denied, that the acrimony of the humours was the cause of ordinary diseases; and, in proof, stated that the administration of acids, alkalies, and other acrid substances, failed to produce effects similar to those observed in the majority of diseases. Concluding, with certain Philosophers, that an immaterial principle is the cause of all the movements observed in the human body, he referred the phenomena, that occurred in health, to what he termed the "Soul"; while its perturbations produce those that arise in disease.* This was only another term for the Archæus of Van Helmont; while his remedies, like those of the latter, consisted of only two classes—calmants and stimulants.

A still greater blow at the chemical theory was struck, shortly after this, by other and different hands. The profound discoveries of Galileo in the 16th century, and of Newton and Descartes in the 17th, had not only given an impulse to the physical and mathematical sciences, but they had been placed on a new and solid foundation. It was to have been expected, therefore, that their light would be reflected back on the science of Medicine, which has always been influenced by every revolution, and by every change in the sister sciences. Such was the case. A new theory, called iatro-,* or medico-mechanical theory made its appearance,—a theory which the discovery of the circulation of the blood tended to encourage.

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* Theoria Medica vera, p. 602, and Dissertatio de *αυτοκρατια* naturæ. Halæ, 1696.

Sartorius, a Professor at Padua, at the commencement of the 17th century, was the first to apply the laws of Statics to the functions of the living body: soon afterwards, Bellini attempted to explain the muscular movements by the laws of Dynamics. His disciples, termed iatro-mechanics, compared the globules of the blood to solid bodies. These, they considered, produced certain effects—such as heat—by their propulsion or friction against the coats of the vessels. But the Iatro-mathematicians, or Cartesians, were disciples of Descartes. According to this Philosopher, matter contains two principles: one that thinks, the other that extends; and to these two principles he referred the orgin of all things. As he considered the force and the movements in all bodies as immaterial effects; so, his disciples explained all the phenomena, in the living body, by the figure and the movement of the atoms of which it is composed. They therefore added plates to their works, showing the form of the salts, their angles, and the changes which they underwent in their conformation. Physiology thus became

* ιατρος, Medicus.

a branch of Physics; and the human body a mechanical rather than a vital machine. But we have not finished with the physical and the mathematical sciences.

Boyle, concluding that the first elements of all bodies are atoms of different forms and sizes, proposed what has been termed the "corpuscular philosophy;" which was applied, by him and others, in order to account for the phenomena and the functions of the living body. Leibnitz, also, adopting the atomic theory—which was merely a revival of that of Asclepiades—maintained that these atoms, which he considered could be divided *ad infinitum*, were organised. It was an illusion to say, there was inorganic matter in the human body. Glisson, also, a Professor in the University of Cambridge, considered that all matter had a principle or nature of its own, which was the cause of every movement. Life, therefore, was merely the action of this principle: death, its dissolution.* After this, several English writers—as Pitcairn, Cole, Cheyne, etc.—captivated by the discoveries of Newton, referred most of the phenomena in the human body to the laws of attraction and repulsion: while they attempted to explain, by algebraic formula, the functions of the living body. Hoffman adopted a somewhat similar theory, as did Boerhaave.†

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* De Ventriculo et intestinis. London, 1677.

† Hoffman was a disciple of the chemical school at first. His conversion, or apostasy from this doctrine, was ascribed to Boyle, whose acquaintance he made during a visit to England.

The adoption of these theories, as a matter of course, produced a like change in the treatment of diseases. As the chemical School attributed diseases to changes in the blood—to acid fermentation and so forth—the mechanical and mathematical schools referred them, or, at least, fevers and inflammations, to stagnation of the circulation, and thickening of the blood in the branches of the capillary system, produced by irregularities in the movement, or flow, of the circulating fluid. The object of treatment, therefore, was to remove these obstructions and irregularities in the circulation: but no regular or systematic method of treatment was adopted: it depended on the caprice or the opinion of each individual. Pitcairn laid down the following axioms: "All diseases, in the human body, arise through changes of matter and of its motion. . . . . The cure of all diseases ought to be effected through changes, properly directed, of matter and its movement."* But as the theory was false, the treatment adopted, in accordance with it, would necessarily be equally fallacious. Hence, although Baglivi attempted to explain all the functions of the body by a reference to mechanical laws, he drew a distinction between theory and practice. Despairing of giving more certainty and precision to the treatment of diseases, he avowed himself to be a disciple of Hippocrates, and advocated the same mode of treatment. Hoffman concluded, that the derangement in the functions of the human body could only be above or below the normal state. In the former case, spasm was produced; in the latter, a state of atony—to one or other of which conditions all diseases were to be referred. Hence, in diseases attended by spasm, anti-spasmodics were employed, as, also, bleeding and evacuants. Tonics were resorted to in the diseases characterized by atony; and alteratives in those of a mixed character. Boerhaave adopted the same ideas, and the same practice. The reputation of these two Professors for learning and skill, as well as the tendency of the age towards the physical sciences, caused their theories to be generally adopted: to the exclusion not only of the chemical theories, and that of Stahl, but, also, of the doctrine of Hippocrates. There were, however, a few exceptions to this general apostasy from the primitive faith, and from the practice of the Apostles of the Medical Art. The two most celebrated were Sydenham and Baglivi—the former termed the English, and the latter, the Italian Hippocrates. Sanctorius published a work, in the 17th century, in order to defend the doctrine of the ancients: as, also, did Hoffman (George) that of Galen. Prosper Martian again wrote a work, and a learned commentary, in favour of the Hippocratic doctrine and practice. But another serious blow was struck at both, soon after, by the promulgation of other and subsequent theories.

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* Elementorum medicinæ brevis conspectus.

In the middle of the 18th century, medical doctrine and medical practice underwent a complete change by the labours of two men in different branches of the healing art—Physiology and Pathology. These individuals were Haller and Morgagni. Up to the time of Haller, Physiology had made little or no progress; but it was then placed by this profound Thinker on a fixed basis. His work, in fact, notwithstanding all the discoveries that have since been made in this branch of Science, is still a standard one. Having instituted a series of experiments, in order to elucidate the phenomena connected with the animal functions, he came to the conclusion, that the muscular fibre possesses a principle which he termed irritability.* Glisson, whose opinions have been already referred to, had propounded a similar doctrine nearly a century before; † while Carter soon after inferred, that this principle was not confined to the muscles, but was shared by every part of the body. Winter, also, a Professor at Leyden, shortly before the time of Haller, adopted the theory of Glisson. He concluded, that all the fibres of the body possessed this principle—the nerves having only the power of exciting it, and setting it in motion.‡ Haller, on the contrary, concluded, that the muscular fibre alone possesses this principle; which, as he attempted to prove, is independent of nervous influence. Promulgated by a man of great learning and reputation in his Profession, this Theory, so plausible and intelligent, was speedily adopted by a majority of the Profession in all countries. It has also continued up to the present time, for while all the preceding theories have been almost entirely abandoned, the Hallerian theory has served as a foundation, with nearly all modern writers, for the establishment of the doctrine of fever and of inflammation.

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* Mémoire sur la nature sensible et irritable des parties du corps animal. Lausanne, 1756.

† De ventriculo et intestinis. London, 1677.

‡ De Irritabilitate: Leid. 1748.

As a matter of course, the theory of Haller had an important influence on the treatment of disease. Instead of Fever and inflammation being caused by obstruction and congestion in the capillary system, these affections, or the phenomena that accompanied them, were considered to be due to irritation and excitement. A purely antiphlogistic treatment, therefore, was adopted in all fevers, in inflammations, and in all diseases supposed to depend on an inflammatory condition of the system. It gave rise, in fact, to the Sangrado method of treatment so general at one time. The propriety of this treatment appeared to be strengthened and confirmed by the result of the pathological investigations, that were undertaken about the same time.

Various Physicians, from the most remote periods, had examined the bodies of men after death, in order to ascertain the causes of disease; but this branch of medical science had been almost entirely neglected, from the time of the destruction of the medical school of Alexandria, until the middle of the 17th century. The first observations that were made, or, at least, published, on pathological anatomy were by Nicholas Fontyn, a Professor at Amsterdam.* These were followed by several other works by different writers, and, at last, Bonet published a collection of the works and observations that had been made on this subject up to that time.† These labours, however, had been of little use in the elucidation of the causes of disease. Thus Bonet, when he met with Polypi in the body, considered that they were the cause of death: while Bennet attributed the death of Phthisical patients to adhesions of the lungs with the Pleura‡—both writers having fallen into the too common fallacy of mistaking effects for causes. It was reserved to Morgagni to erect, on this foundation, the true edifice of pathological anatomy; although he, with great modesty, proclaimed his work to be merely a continuation of, and a commentary on, that of Bonet. Not only may Morgagni be considered as the Founder of this branch of the science, but his work is still a Text book for all Pathologists. He attempted to ascertain, by means of pathological anatomy, not only the seat but the cause of diseases; while the facts adduced by him had the effect of localising many diseases, previously considered to be due to general causes—such as Paralysis and Asthma. Instead of attributing disease to the state of the humours; to derangement of the Archæus; to mechanical causes or to chemical agents, it was referred to the solids—to inflammation, and to changes in the different tissues and organs. This theory was not only applied to the generality of chronic complaints, in which the above pathological conditions were the most apparent; but it was also adopted by certain writers, in order to account for the production of fever, which, up to this time, had been considered essential, or idiopathic. Ræderer and Wagler of Göttingen and Sarcom of Naples, in 1761, drew attention to the pathological condition of the digestive tube, in the fevers that prevailed at that time; they concluded, that these changes, and the inflammation that preceded them, were the cause, not the effects of the disease. These conclusions laid the foundation of a modern theory of disease—that of M. Broussais—which will be presently considered.

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* Observationum variorum analecta. 1641.

† Anatomia practica rationalis. Amsterdam, 1688.

‡ Sprengel. Loc. cit. vol. 5, p. 10.

Although several writers had inferred, contrary to the opinion then generally entertained, that the irritability of the muscular fibre was not independent of the nervous system; it was reserved to Cullen to found the most complete doctrine of Solidism, that had been previously proposed. Concluding, that the remote causes of disease were of a sedative nature, he further inferred, that the first effect of their operation was on the Brain—the energy of which became impaired. Hence, the first link in the chain of causation, according to this doctrine, was debility—the other effects which are observed in fever and inflammation being referred to re-action, the consequence of the previous depression. As a result of the adoption of this theory, this celebrated Professor avoided Evacuants—more particularly purgatives—and depended principally on Tonics and Stimulants. The position of Cullen, as Professor of the Edinburgh School, naturally caused his doctrines to be more widely made known, and to be more generally received, than would otherwise have been the case. His theory of disease became, in consequence, very generally adopted in England; Gregory, Macbride, Musgrave, and other writers drawing similar conclusions on the subject. On the Continent, his opinions were only partially adopted; but they have been advocated by De la Roche and Berlinghieri, a Professor at Pisa, who, nevertheless, charges Cullen with many contradictions.

Brown, a Contemporary of Cullen, and a private Teacher in the same School, but not a Professor, proposed a different theory; one that excited the opposition and ridicule of Cullen, although it led to nearly the same method of treatment. This writer referred all the phenomena of life and of disease to an unknown principle, which he called excitability. "Animals and vegetables," he observes, "are endowed with a principle, the nature of which is unknown, and which is named excitability."* The powers, or Agents, which support life, acting on the "excitability," produce, when they act with energy, the *Sthenic* diathesis; but, when the exciting powers have occasioned a still higher amount of excitement, they exhaust the excitability, and produce *indirect* debility, or the *Asthenic* diathesis. The same result is produced by the deficiency or the abstraction of the natural stimuli, as, also, by treatment, or by the employment of improper agents: this he called *direct* debility. He therefore concluded, that "all diseases of the body are occasioned by too much, or too little stimuli;" and he classed them, therefore, in two grand divisions, the *Sthenic* and *Asthenic*. This, as may be remembered, is merely the doctrine of Themison—the *strictum and laxum*. This opinion, that all diseases are to be referred to two classes—the Sthenic and the Asthenic—induced him and his disciples, both in this country and on the Continent, to recommend antisthenic remedies in the one class, and sthenic ones, or stimulants, in the other. As an antisthenic, cold, he considered, was one of the most powerful. The application of cold, therefore, was to be extended to the whole range of predisposition; to the whole circle of diseases depending upon a sthenic diathesis—even in that of small-pox. On the other hand, having concluded, that Typhus, putrid and petechial fevers, are asthenic diseases, he added: "It is, then, debility alone that is to be regarded in the cure; and stimulant and tonic remedies alone that are to be administered." The doctrine of Brown was only partially adopted in England, at the time, but it was received with great favour in Italy; and his mode of treatment became the general and dominant one for low, adynamic fevers. It was reserved for modern times, and for the present day, to witness the more general adoption, in England, of the Brunonian treatment, in asthenic diseases at least, as will be more particularly pointed out shortly. But, although it was in Italy, that the stimulant mode of treatment was carried out to the greatest extent, it was precisely there, that this method received its greatest blow.

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* Elementa Medicinæ. Edin. 1780.

Rasori, an Italian Professor of some note, translated Brown's work into Italian, and became one of his most zealous disciples.* A petechial fever, of the true typhoid character, having broken out at Genoa, an opportunity was thus afforded of putting Brown's

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* The observations of Brown were translated into Italian in 1792 and the Elementa in 1796.

Method of Treatment to the test of experience. The disease was characterised by a *small*, obscure pulse; rarely strong or full, and, sometimes, singularly weak and *indiscernible*, at the commencement of the attack. The pulse did not improve even in the febrile stage; while fainting, and other symptoms, showed that the vital powers were greatly depressed at this stage, as well as in the former. In the more severe cases, the eruption was petechial; in the slighter, miliary; while, in nearly all, there was either delirium or stupor. Here then was a fever of a typhoid character, which, according to Brownism, indicated a state of the most complete Asthenia. Like a true disciple, Rasori adopted the treatment recommended by Brown, and based on his theory. Fancying, however, that the symptoms were aggravated by this treatment, he resolved to adopt the *experimentum crucis* by employing other remedies, and the opposite mode of treatment. At first, he contented himself with abstaining from stimulants, and ordering cooling drinks, etc.: but he soon employed a more active treatment. He took blood from the arm, once or twice; and administered Tartar Emetic in large quantities.* From the time that he changed his treatment, Rasori lost few patients: although the disease was very fatal with other Practitioners, the majority of whom were Brunonians. This is shown from the fact that, from April to October 1800, there were 7,810 deaths in Genoa. Sydenham had some reason, therefore, for saying, that the false idea of malignity has been more destructive to human nature, than the invention of gunpowder. This remedy—tartar emetic—afterwards became the prevalent one in Italy for the treatment of all fevers, excepting intermittent fevers; and, also, for all cases of acute internal inflammations. It has since been adopted in other countries, and especially in France, for the treatment of internal inflammations, more particularly of the lungs and air passages. This mode of treatment became modified subsequently, for although the antiphlogistic method still continued, it was unaccompanied by evacuants, or purgatives; at least by French, and some other Practitioners.

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* Storia della Febbre Petechiale de Genova negli anni 1799 and 1800.

It has been before stated, that several writers, in the middle of the 18th century, had ascribed the production of fever to inflammation of the mucous membrane of the intestines. In 1805, M. Gariel contended, in an inaugural Thesis, that Fever was not essential, or primitive, only a *symptomatic affection*. In 1813, a work was published by MM. Petit and Serres, on a fever that they named *entero-mesenterique*, being referred by them to a lesion of the intestinal canal. This conclusion was drawn in consequence of the primary symptoms being referrible to derangement of the *primæ viæ*, and from the pathological conditions observed after death. "I can therefore conclude, with truth," observes M. Petit, "that the intestinal affection was the primary disease." Inferring, however, that the lesion of the intestinal canal was not produced by a state of active inflammation, or from enteritis; and considering that the treatment should be directed to the general, rather than to the local, effects, M. Petit administered tonics and stimulants. Baglivi, before this, had placed the seat of fever in the Mesentery, and Sylvius in the Pancreas. There is, in fact, hardly any part of the body, the morbid, or inflammatory, condition of which has not been considered to be the cause of fever.* Clutterbuck concluded, that the Seat of Fever was in the Brain: but it was reserved for M. Broussais to complete the edifice raised on the pathological theory of disease, the foundations of which only had been laid by others. "All was prepared," remarks one writer, "for a new medical revolution; it only required, so to speak, the advent of the Medical Messiah, who was to accomplish this regeneration. This Messiah appeared, at last, under the name of Broussais. A bold generaliser, an observer as skilful as profound, this illustrious master levelled a mortal blow at the doctrine most generally received, by the publication, in 1816, of his celebrated Examen." *

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* Galen concluded long before, that fevers arise from inflammation, and particularly from pleurisy and pneumonia.

The theory of M. Broussais is, that all fevers, and the majority of diseases, are produced by inflammation of the mucous membrane of the intestines. "All the essential fevers of authors," he remarks, "may be ascribed to a *gastro-enterite*, either simple or complex." † It is, also, by an acute *gastro-enterite* —the first effect of the contagious agent—that Small Pox supervenes. And it is by the same, and by an acute inflammatory catarrh of the eyes, nose, throat, and bronchi, that Measles and Scarlatina make their appearance. This doctrine, although not entirely new, met, as usual, with considerable opposition at first: but it was not long before it became generally adopted in France and on the Continent. Hence, as the writer before quoted remarks, "the essential nature of fevers has disappeared, like a vain phantom; and they have, at last, entered into the class of Phlegmasiæ, as M. Broussais predicted in the Preface to the 1st Edition of his Work, for ever memorable." Like all other theories proposed in order to account for the causation of disease, this doctrine had an important influence on the Treatment. By enlarging the circle of Inflammations (*phlegmasiæ*), he extended the application of the antiphlogistic method; and by localising the diseases that were before considered general, he raised the value of local emissions of blood. "To have substituted leeches and emollients for emetics, purgatives, tonics and stimulants, in the treatment of the fevers termed essential, is, assuredly, a great revolution in therapeutics."* And the same author adds: "A person may predict, without being a very wise Prophet: 1st, that the localisation of fevers, called essential or continual; 2ndly, that the simplification of the treatment; and, 3rdly, that the substitution of the antiphlogistic method for the tonic, stimulant, and *incendiary* method, in the treatment of fevers called adynamic, or ataxic: we may prophesy, I repeat, that these three grand ideas will assure to him, who has conceived them, and caused them to be adopted, an eternal glory." So far from being eternal, this glory did not last very long. The advent of the epidemic cholera, and the adoption of the same mode of treatment in this disease, not only showed the fallacy of the doctrine, but the result was so disastrous, that M. Broussais abandoned both his treatment and his practice in despair, With other Practitioners, and especially with those who adopted the theory of irritability and inflammation, as the sole or principal causes of disease, the old antiphlogistic, or heroic, method of treatment was adopted, viz., bleeding, cupping, blisters, evacuants, etc.—a system which, when carried to excess, was productive of as much mischief as good. This method of treatment was favoured and encouraged by the theory of the late Dr. Clutterbuck, that nearly all fevers were caused by inflammation of the brain or of its membranes; although several writers, long before, had pointed out, that no inflammatory appearances could be detected within the head in cases in which the cerebral symptoms, during life, were very prominent and marked.

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* Philosophie Médicale ; p. 84.

† Examen. des Doctrines Médicales. Proposition 109.

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* Philosophie Médicale; p. 92.

The antiphlogistic method of treatment has now disappeared, and, as generally happens, has been replaced by a very different theory, and the opposite mode of treatment,—by stimulants, instead of by depletions. This, at least, is the case for the ordinary fevers met with in England, and for certain cases of inflammation. One of the strongest advocates for this mode of treatment was the late Dr. Todd, who trusted almost exclusively to alcoholic stimulants in the treatment of fevers and some other diseases.* "Alcohol," remarks this writer, "is a remedy whose value can scarcely, I think, be overestimated; and one upon which, when carefully administered, I rely with the utmost confidence in a great number of cases of disease, which are at all amenable to treatment."† That these and other stimulants can be resorted to, with advantage, in low, adynamic forms of fever, when combined with other remedies, is undoubted; but their employment as adjuvants, in small doses, and as remedies, alone and uncombined with any other, and in large quantities, are very different things. Alcohol, in certain cases—in quantity not larger than has frequently been given in disease—is poisonous. When taken in poisonous doses, death is produced in three different ways. (1). Suddenly, and in a few minutes, from coma. (2). From apoplexy, or extravasation of blood on the brain. (3). From congestive apoplexy, without extravasation—a result that is generally observed after a certain interval, and when there has been an apparent recovery. As some of these effects, and, also, those observed in ordinary cases of intoxication,—such as congestion of the brain, coma, and delirium*—are precisely those that are observed in low, adynamic, and other forms of fever—thus showing the analogy that exists between the two states—to administer alcohol, in fever, in poisonous doses, as, for instance, a pint of brandy daily, appears to me to be not scientific but empirical treatment. That it is not homicidal treatment, can only be referred to the following circumstances. Either the remedy is not absorbed into the blood—Majendie having shown

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* Clinical Lectures, p. 439. Ed. 1859.

† Dr. Todd gave Brandy to the extent of 12 ounces, in the twenty-four hours, combined with Ammonia and Chloric Æther,

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in Typhus fever, in Small Pox, Pneumonia, Erysipelas, and the *delirium* of acute diseases.

* The spirit called "Samsoo," in China, and distilled from rice, does not produce stupor, or coma, but furious maniacal delirium.

that absorption is suspended, when the venous system is in a state of congestion*—or, else, the nervous system is so depressed, that stimulants do not produce their ordinary effect in these cases. This inference would appear to be confirmed by the fact, that stimulants fail precisely in those cases in which we should expect to find, that they were the most beneficial. "There are many cases," remarks Dr. Stokes, another great advocate for the employment of stimulants, "of essential diseases—of puerperal fever, pyæmia, malignant scarlatina, small-pox and others—in which, though the vital strength *is fearfully prostrated,* for some reason that we cannot explain, stimulants are too often powerless for good. They are never, in fact, in such cases, followed by the same happy and almost heroic effects which we so often witness in typhus fever." (Lectures on Fever, p. 338.) Although there may be no injurious results when first taken, the remedy may accumulate in the system, and produce dangerous, if not fatal effects, when re-action takes place.

It would thus appear, that the practice of medicine, so far from being a progressive science, has been merely a science of revolutions. If we look back at the preceding history, we shall be surprised to find how theories, that were abandoned and, apparently, lost, have been revived again and again. "Our notions in Physick," says Dr. Barker, "change with our Philosophy, and, at last, we return to our old ones again." In fact, medical theorists, as Lord Bacon remarks, have been going round the same circle, like the horse in a mill, and continually arriving at the point from which they had started. Unfortunately, these revolutions have not been attended with any advantage, for there has been, as his Lordship quaintly adds: "much iteration and small progress." As another writer has observed, hypotheses in physical science are few and fair to behold; but, "in the science of life, they are without number; their name is legion; and, in most instances, they are as remarkable for their complicity, clumsiness, and improbability, as the theories of physical science are for the opposite qualities."[*] This is not all. Each revolution in theory has produced a corresponding change in practice. Each system, as Bichat has remarked, which, by turns, has prevailed in the Science of Medicine, has been reflected back on the materia medica, or on Therapeutics: "Into what errors have not men allowed themselves to be drawn in the use, and in the designation, of remedies. When the theory of obstruction was in vogue, deobstruants were employed. Incisifs[†] sprung into use when a thickening of the humours was considered to be present. When it was considered necessary to blunt the acrid matter, *inviscans*, *incrassans*,* etc., were invented. Those who only saw relaxation or tension of the fibres in diseases—*laxum and strictum*—made use of astringents and relaxants. Cooling and stimulating remedies were employed by those who, in diseases, had an especial regard to an excess or deficiency of heat, etc." If the theory is erroneous, the treatment based on it would necessarily be so also; but the result, in the latter case, would be very different to that in the former. With the one, the errors might be corrected in time; with the other, the consequences—the loss of a certain number of lives or injury to the health—would be irreparable. In addition to this, as the promulgation of a new doctrine did not banish the others, different modes of treatment have prevailed at the same time. There has been no fixed standard of treatment, no recognized system to which Practitioners could refer, and which they felt bound to follow. The consequence of this is a state of empiricism, which has prevailed, to a greater or less extent, up to the present time. It may, in truth, be said, that all diseases are treated empirically at the present time, and by the whole Profession. This was shown, most conclusively, during the prevalence of the

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* M. Claude Bernard states that, in Typhoid fever, liquids and other substances taken into the stomach are not absorbed, in consequence of the inflammatory state of the intestinal canal.

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[*] The Philosophy of Medical Science. By Elisha Bartlett, M.D. Philadelphia, 1844.

[‡] *Remedia incidentia,* remedies which possess the supposed property of cutting the viscid humours.

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* *Remedia incrassantia*, which are supposed to possess the property of thickening the humours, when too fluid.

Epidemic Cholera in England. The disease, it is true, was a new one; but it had existed 13 years in India before it reached Europe; while three voluminous Reports by the Medical Boards of the three Presidencies, containing the opinions and conclusions—both theoretical and practical—of the majority of the medical men in India, had been drawn up and published a few years after the appearance of the epidemic in that country. The physiology of the disease, also, had been ably and fully entered into and discussed by Mr. Bell, in his work on Cholera. There could be no reason, therefore, why this *nova pestis* should not have been treated on some general principle, even if it could not be combated successfully. Instead of this, each Practitioner had some particular remedy, or plan of treatment, of his own; while the remedies employed were of the most opposite character and properties —blood-letting, narcotics, stimulants, purgatives, astringents, acids, alkalies, and, frequently, in combination the one with the other. Well might a talented Reviewer—the late Dr. James Johnson—exclaim: "Amidst the variety of remedies presented to our notice, we feel like a hungry guest, with a splendid bill of fare: each article tempts, but which shall he prefer? One Gentleman cures cholera with cold water, another removes it with hot; a third puts it to flight with calomel and opium; a fourth exclaims, that calomel and opium are poisons, and drenches with salt water; a fifth exclaims against the absurdity of salines by the mouth, and throws them into the veins; a sixth—but why should we go on?" It was not only unnecessary, but the task would have been somewhat difficult, for one writer —Dr. Favre—who has taken the trouble to count them up, states that 5,000 different remedies—*including nearly every known poison*—have been resorted to for the treatment of this modern scourge. But of all remedies and all poisons, the employment of opium, and in heroic doses too, in a disease in which all the organic functions are suspended, and which acts as a narcotic on the only system that remains intact—the cerebro-spinal—is the most extraordinary. Can we wonder then at the frightful mortality, which has attended the employment of opium in heroic doses; or, at the fact, of which there can be no doubt, that cholera patients have been buried alive, while in a state of narcotism?* It has been facts like these which has made one writer, Dr. Parkes, who had the advantage of treating the disease both in India and in Europe, exclaim: "The antidote to this tremendous poison has not yet been discovered; and the resources of modern European science have opposed its destructive action with as little effect as the untutored efforts of the most barbarous nation to whom its ravages are known. The efforts of European science have indeed, as it appears to me, in many cases proved hurtful."*

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* For the proof of this, vide Introduction to "Epidemiology."

The natural result of such a state of things has been to produce in Medicine, what has always been observed in Religion, when false doctrines have prevailed, a state of scepticism respecting the beneficial operation of remedies. This feeling has been manifested at various times, lately as well as formerly, not only with the public but among members of the Medical Profession. In a sermon preached before the University of Cambridge, on the day of general thanksgiving, Nov. 15, 1849, one of our profoundest philosophers, the late Dr. Whewell, gave utterance to this feeling in the following words :—"For now, after this pestilence has twice stalked through the land, the wisest of our physicians presume not to say, that they know more of its nature and origin, or of the means of resisting its invasions, and healing where it smites, than they knew when first it appeared among us. And thus, while men were loudly boasting of their knowledge of the human frame, and of the laws of life, and of the means of directing the vital powers, so as to cast off disease, and procrastinate death, the very first event which occurs, deviating from the common and familiar course of things, is utterly beyond the circuit of the field to which this knowledge extends." "What," exclaims Dr. Rush, "have

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* On the Algide Cholera.

Physicians, what have Universities or Medical Societies done, after the labours and studies of many centuries, towards lessening the mortality of pestilential diseases? They have either copied or contradicted each other in all their publications: for plagues and malignant fevers are still leagued with war and famine, in their ravages upon human life."* Look, for instance, at the measures that have been adopted for the Prevention of disease. Although ushered in with a great flourish of trumpets, and although we were told, that epidemic diseases would hereafter be banished from the earth, what has Sanitary Reform effected after a trial of 50 years? As if in mockery of these vain promises, and of the puny efforts of man to prevent the return of epidemic and other diseases, the general rate of mortality has actually been higher since the introduction of these so-called sanitary measures than before; while *three* new diseases—the cholera, diphtheria, and typhoid fever—have made their appearance in the interval. More than this, these very measures have actually been provocative of disease in several instances.† This result is not, perhaps, so surprising as the failure to cure diseases, after they have appeared; and of the expression of that conviction by those who have had the most experience in the practice of Medicine. The celebrated

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* Medical Inquiry.

† For the proof of these assertions see Epidemiology, Chapter, Modern Theories; and Causation and Prevention of Disease.

Professor Boerhaave, referring to the false theories, and the no less false modes of treatment, that were based on them, remarked long since: "If we compare the good which half a dozen true disciples of Esculapius have done, since their art began, with the evil that the immense number of doctors have inflicted upon mankind, we must be satisfied, that *it would have been infinitely better, if medical men had never existed*"! And Dr. Haughton states: "It is but recently, that a distinguished Physician,* and Editor of the first Medical Review in the world, after practising the art of medicine for half a century, openly declared, that a large proportion of recoveries, which took place in ordinary practice, do so, *in spite of the treatment* adopted; and that many cases prove fatal *in consequence of the medicine given*."† Another no less experienced Physician, the late Dr. James Johnson, Editor of the Medico-Chirurgical Review, gave vent to the same feelings, in still stronger terms. "I declare it as my conscientious opinion, founded on long experience and reflection, that if there was not a Physician, Surgeon, Apothecary, Man-midwife, Chemist, Druggist, nor drug, on the face of the earth, there would be less sickness, and less mortality than now prevail." When Dr. Johnson gave utterance to these reflections, he must have meant the medical, not the surgical, treatment of diseases by surgeons; surgery being a true science, in the present day, and worthy of ranking with any other. But as regards medicine, we may ask, with M. Claude Bernard, is it a science, yes or no? "It is," adds this writer, "allowable to say, that medicine is a science, and even one of the most ancient, since Hippocrates, who lived 460 years before Jesus Christ, is regarded as the Father and the Founder of it. On the other hand, we may yet affirm, that after 23 centuries of practice and of teaching, we have still to ask, if this science of medicine really exists. It presents, in fact, this melancholy spectacle, that ignorant men and quacks are more successful in practice than learned Physicians, who have passed all their lives in its study. There are then reasons for believing, that medicine has not yet become a science (*n'est pas encore faite*): for it never occurs in the fixed sciences for a savant and an ignoramus to be confounded together." Hence, in his introductory Lecture, at the College of France, in 1847, this renowned Professor uttered these memorable words: "*The science of medicine*, that I am appointed to teach you, *does not exist.* The only thing that we have to do is to prepare the foundation for future generations; to create the physiology on which this science may be hereafter established."* Another writer† allows that medicine may be a science, but only a science of conjecture; while a third*—one of the most distinguished Professors in France—declares that it is not a science only an Art—an empirical art.†

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* Sir Charles Forbes is here alluded to.

† On Vital Force, in London Medical Review, vol. 3, p. 64, 1862—3.

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* Lecons de Pathologie Experimentale, pp. 455—6. Paris, 1872. † Cabanis, *Du degré de certitude en medicine*, Paris, 1803.

To what then are we to refer the present lamentable state of the Art of Healing—that Art which caused some of its first Professors to be elevated into the rank of a God? One of the reasons has been the adoption of so many new theories, and the laying aside of that simple and scientific mode of treatment pointed out by the Father of Medicine himself. "Our misfortune," remarked Sydenham, referring to the practice in his day, "arises from having long since forsaken our most ancient, and most skilful guide, Hippocrates, and the ancient method of healing, based on a knowledge of conjunct causes—these being deduced with certainty; so that the art, which is practised at the present day, having been invented by shallow-minded men, is one of babbling and of talking, rather than of healing." * Mr. Adams, the learned Commentator of Paulus Ægineta, says, in one of his notes, in reference to this doctrine: "Notwithstanding its being little in repute, I am not afraid to declare that, in my opinion, it accords better with the phenomena of diseases, and is a more successful guide to practice, than any hypothesis that has been recently advanced." And Sprengel, speaking of the prevalence of injurious systems in medicine, adds: "Physicians preferred to sacrifice their patients to the fashion, and thus lead them to the grave, rather than to restore them to health, by following the method of the ancients."† One writer assigns the following, as the cause of this departure from the primitive doctrine. "The greater number of Physicians, we may say, spoilt by the theoretical education they have received, and too impatient, wish to anticipate the process of nature, and the natural phenomena. . . . The same as when taking an egg, from under a hen, the temperature of which is 37½deg. (cent.), you would hasten the hatching, by giving it a few degrees more of heat; either you kill the chicken, or you make the egg hard. In a mul-

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* Trousseau, *Conférences sur l'empirisme.* Paris, 1862.

† The preceding opinions would seem to be shared by the majority of English Practitioners, judging from the practice, now so common, of sending patients abroad, for what is commonly called "change of air." Either, therefore, they must think, that the efforts of Art are useless, in a great many diseases, or, else, that the climate of England is a most pestiferous one, instead of being, what it really is, and always has been, one of the most healthy climates in the world. Why Invalids should be induced to leave the comforts of home, and the quiet of English life, for the discomforts of foreign Hotels, and the noise, the fatigue, and the risks of rail-road travelling, is an enigma past my comprehension. We do not find that French or Italian or Spanish Invalids are sent to England, although the change would be more beneficial to them than it is to Englishmen, when they visit the Continent. The change, in fact, in the majority of cases, is detrimental rather than beneficial, of the truth of which I could adduce numerous examples.

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* Opera Omnia. Ed. Sydenham Society, p. 15. † Vol. 5, p. 69.

titude of circumstances, the Physician does the same, and it is sad to say so. By the fact, that he does not observe the natural phenomena with sufficient care: that he does not early learn the course and the peculiarities of diseases; he becomes incapable of recognising the action of the remedies that he orders; and all the deductions that he draws are valueless, for the first notion—and the most important—is to know how the disease progresses independently of the action of remedies." *

Another reason is, that we have remained, up to the present time, in complete ignorance of the immediate and proximate cause of the majority of diseases. If we are ignorant of the cause of a disease, it is impossible that we can treat that disease either scientifically or successfully. This is a truth, that was acknowledged by some of the immediate successors of Hippocrates, as we are informed by Celsus, who, while referring to their opinions, added: *Neque enim credunt, posse eum scire, quomodo morbos curare conveniat, qui unde hi sint ignoret.*† Not only may we refer the present uncertain state of the practice of medicine principally to this cause; but it is to this, also, that we must ascribe the variety and the number of theories, which have prevailed in the Science of Medicine. Had the cause of the morbid phenomena been as well and as clearly defined as the healthy phenomena and functions of the human body, the practice of medicine would have long since been placed on a sure and stable foundation. Instead of this, it has been like a weathercock, subject to continual change, and the sport of every wind, no matter from what point of the compass it might blow.

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* Prof. A. Trousseau, *loc. cit.*

† De re Medicina. Liber primus.

The cause of the failure hitherto of medical Theorists, in their attempts to unravel the mystery that hangs over the subject, has been the wrong method they have pursued. Instead of following in the path pointed out by Bacon and Newton, who, guided by *analysis*, endeavoured to search out the true course of nature, and to discover her laws, medical Theorists have pursued the opposite course. They have first formed their theory, and have then endeavoured to explain the effects, in accordance with their pre-conceived opinions. They thus imitated Descartes, who deduced Laws of Nature from arbitrary ideas, and afterwards applied them by *synthesis* to particular cases. But the main business of natural philosophy, said Newton, is to argue from phenomena, without feigning hypotheses. "Following in the path of induction, Philosophers and Physicians would, to employ the comparison of Bacon, have resembled Bees, which gather honey from flowers at every season; assimilate it to their nature, and thus prepare it for the use and the pleasure of man: instead of which, the advocates of the modern systems, like useless spiders, draw from their own body the feeble web, that only serves them to entrap the insects in their obscure den."* In this way, the first acquisition of knowledge, in the collateral Sciences, was of injury rather than of benefit to medicine. Instead of the former being the handmaids to the latter, medicine was made the slave to every other Science. Thus, the Anatomist endeavoured to ascertain the cause of disease, not by studying the morbid phenomena during life, but, by examining the body after death; although uncertain whether the effects then observed existed during life, or at the commencement of the attack. Independently of the false conclusions that may thus be drawn, the very tendency of pathological investigations is to make men sceptical of the power and efficacy of medicine. On opening a body, they observe certain effects, that the treatment adopted has failed to remedy; and they conclude at once that, in similar cases, medicine will be inefficacious. They do not reflect, that these states may only be the ultimate, not the primary, effects of the disease; and that they might be prevented by early and proper treatment. The very object of treatment, in fact, is, not to remedy effects, that are incapable of removal, but to prevent their supervention; in the same way that we put a patient under treatment to prevent the ultimate result of all—death. Although we are enabled by Autopsy, to ascertain the immediate and direct cause of death in many cases, in others, our researches are entirely frustrated. Sometimes, an organ which, during life, was the most affected, is found, after death, to be perfectly sound, or, at least, not to exhibit any trace of disease. On the other hand, profound lesions are frequently discovered, of which not the least symptom, or indication, existed during life.* The Chemist also, in his Laboratory, imagined, that he could ascertain the changes that occurred in an organized body, during disease, by a reference to the combinations and decompositions that take place in inorganic substances; and that he could control these actions by the same means. "The changes of matter which take place in the midst of the liquids circulating in the system,—the combustions, the decompositions, the syntheses—are subject to the same powers, and are governed by the same laws as the re-actions which the chemist produces out of the body, by the aid of acids, alkalies, heat, pressure, and the other physico-chemical forces at his disposal. But nature never employs such methods in the interior of our organs; the result of their action would be, in effect, the destruction of our bodies."* The Philosopher, also, sought for a solution of the problem in his Euclid, or by studying the Laws of Statics and Dynamics. "As the physical Sciences," observes Bichat, "were perfected before the physiological, it was considered possible to elucidate the latter by combining them with the former; they only produced confusion; it was inevitable, for to apply the physical sciences to physiology is to explain, by the laws of inert bodies, the phenomena of living bodies." At one time, medical men appeared to think more of classifying diseases, according to their pathological condition, than of curing them. The problem of Pitcairn: "A disease being given, to find a remedy," was no longer considered to be the principal object, and the aim of medical Science. On the contrary, Pinel stated, that it showed more of presumption than of knowledge and wisdom to attempt the solution of such a problem. He therefore proposed to substitute another problem for the above: viz., a disease being given to determine its character, and the place it ought to occupy in a Nosological Table.*

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* Sprengel, v. 5, p. 192.

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* Goethe, in his drama of Faust, represents the Devil, seated in the Professor's chair, with gown, etc. In his address to the students, on the study of Anatomy, he exposes the illogical reasoning of this class of theorists in the following lines :—

"For he who seeks to learn, or gives,
Descriptions of a thing that lives,
Begins with 'murdering to dissect'
The lifeless parts he may inspect.
The limbs are there beneath his knife,
And all—but that which gave them life!
Alas! the spirit hath withdrawn,
That which informed the mass is gone;
They scrutinize it, when it ceases
To be itself, and count its pieces;
Finger and feel them, and call this
Experiment—analysis." †

† *Faustus*. Translated by John Anster, LL.D. Dublin 1835.

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* M. Claude Bernard, *loc. cit.* p. 247.

It is time, therefore, that we retraced our footsteps, and returned to the precepts and the practice of the Fathers of the Medical Art, or, else, sought a new and more successful method of treatment than any hitherto adopted. With the experience of ages before us, and with the example of those that have wandered from the right path, and who have been lost in the mazes of doubt, of scepticism, and of empiricism, we ought at all events to be able to adopt, in the present day, a rational, if not a scientific, mode of treatment. Should the result not be commensurate with our expectations, we shall have the satisfaction of knowing, that our want of success is due to the insufficiency of human means, not to the imperfection of the medical art, or to the perversion of the human intellect; to the want of the exercise of those faculties with which man is endowed—the faculties of observation and of inductive reasoning.

With these preliminary remarks, we may now pass on to a consideration of the subject that more particularly concerns us—the treatment of disease. In order to treat a particular disease, rationally and scientifically, we must, as previously remarked, be acquainted with the cause of its production, otherwise our treatment will be alike conjectural and empirical. As, however, we cannot subject the living body to a chemical analysis or an anatomical investigation, our only resource is to analyse the morbid phenomena then present, in the hope of ascertaining, by analogy or induction, the cause of their production. This will be attempted in the following chapter.

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* Nosographie Philosophique.

CHAPTER I.

THE PHYSIOLOGY OF DISEASE.

"WHAT," exclaims M. Claude Bernard, "is disease? This, without doubt, is one of the first questions that presents itself to the mind of a Physician. But none of the definitions which have been laid down, up to the present time, have offered a satisfactory answer. In fact, the greater number of these definitions having been established, *à priori*, are of no value whatever in the actual state of our knowledge.*" If, by the term disease, we mean those peculiar states that are cognisant to our senses, there can be little difficulty in answering the question. In this sense, disease may be defined to be an abnormal condition of one or more of the functions or organs, or of the fluid and solid parts, of the human body. But this definition is not, and ought not to be, sufficient for the Professor of the healing art. It is necessary for him to know the nature of the morbid alteration, as, also, the cause of its production. To arrive at this knowledge, an acquaintance with the various branches of the medical science is absolutely necessary. He must be acquainted, in the first place, with, what is now termed, Biology, or, the Science of Life. This branch of the healing art has been divided into:

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* Loc. cit. p. 126. These Lectures, delivered in the winter of 1859-60, were first published in the *Medical Times and Gazette*, vol. 2, 1860; re-translated into French by Dr. Benjamin Ball, and published by M. Claude Bernard, with some omissions and additions, in 1872.

1st. Anatomy, by which we become acquainted with the form, texture, and condition, of the different parts of the body in their normal state.

2nd. Physiology, which teaches us the functions of the different organs, or parts, of the body, during health—functions on which life depends.

In addition to the above, the knowledge of another branch of the same art is required, viz., the Science of Death, or Pathology. Without this knowledge, we shall be unable to ascertain what the morbid changes are, which occur in disease—the majority of the changes being internal not external.

When acquainted with the preceding subjects, we shall then be enabled to enter into the consideration of another subject; one that has not hitherto been considered as a distinct branch, but has been, and is even now, generally confounded with the preceding, or Pathology. This is the Physiology of Disease, by which we are to understand the study or elucidation of the phenomena witnessed in the human body during an attack. To apply the term Pathology to this branch of the science is a misnomer: it should be restricted to its legitimate use—the morbid state observed in a body after death. As these states are generally the ultimate, not the primary, effects of the operation of the morbid cause, while, in some instances, they are wanting altogether, it is clear that if we wish to ascertain, what is the immediate and direct cause of the phenomena witnessed during states of disease, we must study the effects produced during life, not after death. As the modes of inquiry, therefore, are different, so, also, should the terms be. As Physiology is the word employed to designate that part of the Science, which explains the functions and the phenomena of the living body, during health; so, also, it would seem to be the most appropriate to designate that branch of the healing art, the object of which is the elucidation of the phenomena observed during disease; or, of the laws by which it is governed. "As long," remarks M. Claude Bernard, "as we are ignorant of the *laws* of the phenomena (of disease), it is impossible for us to understand them, or to regulate their course. It is the attentive study of the *mechanism* of disease, which will alone enable us to interfere at the opportune moment." * It is, in fact, the most important branch of Medical Science, without which all the others would be useless, as far, at least, as the ætiology of disease is concerned. Hence the importance attached to the subject by the first Physiologist of the day—M. Claude Bernard. He remarks: "Many persons, in the present day, think that Medicine, as a single Science, does not exist: but that it is rather a *réunion* of various medical sciences. For myself, I frankly admit, that there is only one science in medicine, and that this science is Physiology, applied to the healthy, and to the morbid condition." * M. Majendie also remarks, the Science of Medicine is only the Physiology of Man in a state of disease.

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* Loc. cit. p. 115.

Important as this branch of Medical Science may be, there is yet another, which requires to be known, before we can ascertain what the real causes are productive of disease. This is what the French call *La Science des milieux*, or a knowledge of the medium in which man lives, and of the various agencies which affect him externally; it having been satisfactorily shown, that the majority of diseases are produced by external causes. With these remarks, we may pass on to the subject that will now engage our attention—the physiology, or the immediate, proximate, cause of disease.

In order to ascertain what is the immediate, or proximate, cause of disease, we cannot do better than take, as the foundation of our inquiry, the class of fevers, and for several reasons. In the 1st place, fevers have prevailed from time immemorial, in the same form, if not in the same intensity, as at present, and in all countries, to a greater or less extent. "How great is the prevalence of fever will be apparent," remarks Macculloch, "almost on a moment's consideration, when we reflect that, in all the warmer, and, hence, more populous countries, nearly the entire mortality is the produce of fever."* The same remark will apply to all the countries in the South of Europe. Not only are fevers more general than any other disease, but they are intimately connected with the majority of diseases. In the next place, fevers present three different types—the continued, the remittent, and the intermittent. In addition to this, the last is characterized by three distinct stages, which not only succeed each other with unvaried regularity, but which, after subsiding, return, again and again, at fixed periods. As such, a most favourable opportunity is afforded, not only for studying the phenomena presented during the attack, but, also, for ascertaining the cause of the fever: we cannot do better, therefore, than make the last named disease the basis of our inquiry. By so doing, another advantage will be obtained: the remote, or exciting, cause of ague has been clearly demonstrated, and is now generally allowed. This is the operation of a poison to which the term Malaria (*mal'aria*) has been applied by Italian writers, and that of "marsh poison" by English ones—an agent which is extricated from the surface in particular situations, and at particular times, and thence diffused in the surrounding atmosphere.*

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* Loc. cit. p. 8.

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* Essay on Malaria, p. 6.

Independently of ague, the continued and remittent forms of fever of intertropical, and some extratropical, countries are also referred to the same cause, by all those writers who have made this subject their particular study, and whose opinions are of any value. That these three forms of fever are produced by one and the self-same cause, may be inferred for various and different reasons. Although presenting, apparently, marked differences, the phenomena observed in the one are precisely the same as those in the other forms of fever—the only difference being a difference of degree. A more conclusive argument is, that intermittent fever is frequently converted into remittent or continued, while these last as frequently terminate in intermittent; the variation being co-incident with changes of temperature—continued fever prevailing in the summer, remittent in the autumn, and intermittent in the spring. In other instances, we observe the three forms prevailing, at the same time, in the same locality: continued fever on the plain below, remittent at an elevation somewhat higher, and intermittent at the highest elevation. There is, however, one peculiar and striking difference between the three forms of fever, viz., the periodicity of intermittent fever.

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* Vide. Causation and Prevention of Disease.

This singular phenomenon, although offering so great a temptation to scientific investigation, yet remains an unsolved problem. Comparatively few writers have even attempted its solution, notwithstanding that this disease, unlike many others, is to be found in every part of the world, and at all times. Stahl inferred, that the return of the paroxysm is due to habit—the attack appearing at the usual hours of the repast, breakfast, dinner or supper. He acknowledges, however, that the cause is *inscrutable*. Casimir Medicus, who has given a history of periodical diseases, attempts to show, that the periodicity is produced by the presence of morbid secretions and substances in the stomach and intestines. According to him, nature makes an effort to get rid of these matters, and the commotion is communicated to the whole system. Such an explanation, however, leaves the question of the periodicity altogether unanswered. According to Willis, agues are produced by a *periodical* fermentation of the blood: while Borelli concluded, that they were produced by a periodical irritation of the nerves, the effect of a state of acrimony of the nervous fluid. Torti, who had such valuable opportunities of studying the malignant fevers of Italy, and who has written largely on the subject, considered that both the above causes were in operation. M. Bailly, remarking that animals, exposed to the same causes of disease as man, are only affected with continued fever, while the latter is attacked with intermittent, has ascribed the intermittence to the *erect* posture. The variation caused by the difference of pressure of the abdominal organs on the venous trunks, when in repose and when erect, is considered by this writer to be sufficient to account for the paroxysm.* This hypothesis is refuted by the following facts. In the hospital, Spirito Santo, principally devoted to the treatment of intermittent fever, at Rome, it is, or was, the custom, nay, the invariable rule, to keep the patients in bed until the fever ceases. But this makes no difference in the attack, which has been found to return as regularly as before the admission of the patient, and at precisely the same hour. In addition to this, M. Bailly's conclusion is not exact, as some animals are subject to attacks of intermittent fever. M. Metaxa, Professor of Comparative Anatomy at Rome, has offered another explanation of the difference referred to: this is the thickness of the skin of animals. In consequence, the sweating stage is wanting, and the apyrexial stage, which he infers is due to the transpiration, is not observed. In horses, however, that perspire freely, intermittents, according to this author, are not uncommon. Mr. Royston has also stated, in a letter addressed to Dr. Adamson, that the animals employed in agriculture, in the marshes of Cambridgeshire, were frequently affected with intermittent fever of a tertian type. Pozzi and other authors have also narrated cases of regular intermittents in the horse. Reil imputed the periodicity to the intermittent character of all the organic functions; and Roche to the periodicity of the exciting cause. This, however, is only leaving the question where it stood; or, rather, proposing one problem in order to solve another. Dr. Wilson Phillip remarks: "Concerning the manner in which marsh miasma acts, in producing agues, we can say nothing with certainty."* The most recent writer on the subject—Dr. Copland—says: "The periodicity of morbid actions cannot be explained otherwise than by referring it to a law of the animal economy: and, as those maladies, in which the nervous system is primarily and chiefly affected, are most remarkably periodic, we may infer, that it is especially dependent on these systems."† This conclusion, supposing it to be true, will aid us very little in our inquiry: the only difference would be that, instead of endeavouring to ascertain the cause of the periodicity in ague, we should have to inquire, what is the cause of the periodic action of the nervous system. Besides, as one author, M. Bailly, has rightly observed: "Intermittence is not the constant character of nervous affections, when observed in animals, or even in man, when young and vigorous." Dr. Copland himself admits, that his explanation is insufficient and unsatisfactory, for he adds, in another place; "Intermittence is, as far as we yet know, an ultimate or unexplained pathological fact." * It is desirable, therefore, to ascertain, whether an explanation can be offered of the cause of this singular phenomenon—the periodicity of intermittent fever. For this purpose, we will examine the phenomena presented to our notice during an attack of this disease.

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* Traité Anatomico—Pathologique des Fievres Intermittentes.

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* A Treatise on Fevers.

† Dict. of Practical Medicine, Art. Disease, parag. 156.

The first symptom experienced is, generally, a sensation of cold, which is speedily followed by chills and rigors. The skin at the extremities becomes colourless, wrinkled, and livid, as, also, the mucous membrane of the mouth and lips: the breath is cold, the respiration difficult, and the pulse, in the severer cases, scarcely perceptible. These effects do not, in the generality of cases, last very long—the period varying from a quarter of an hour, in the slight cases, to 3 or 4 hours in the severe ones. On the subsidence of these symptoms, the stage of fever immediately commences, the duration of which, like that of the cold stage, varies according to the intensity of the disease. This is followed by the sweating stage, which is peculiar to ague. To what, then, are we to refer these various effects, so different in the cold to those observed in the hot stage?

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* Loc. cit., Art. Intermittent Fever.

It was impossible for the Father of the Medical Art, unacquainted with the circulation of the blood, to draw correct deductions respecting the phenomena observed in Fever. But according to later writers —those acquainted with the circulation of the blood, and who, at the same time, adopted the hippocratic theory—the two stages of fever are thus acccounted for. "The febrile matter," remarks Sydenham, "on its first production, or introduction into the sanguineous mass, excites it in a certain manner and disturbs it, so that the natural and sensible parts being irritated, and the matter itself endeavouring to escape, chills and rigors are produced in the body."* Hence the production of the cold stage. As regards the febrilc stage, he adds: "Nature, therefore, being by this means irritated, raises a fermentation, in order to expel the enemy with less difficulty; this being the common instrument she uses to free the blood of its morbid particles, as well in fevers as in some other acute diseases."† When the ancient doctrine, or the humoral pathology, was abandoned; and when solidism became the dominant theory, an attempt was made to account for the phenomena in fever in a different way. Van Helmont considered, that the cause of fever is more fitted to excite and derange the archæus than to alter the structure of the parts, or the mixture of the humours. Hence, the cold stage is referred by him to the alarm or excitation of this power: the hot, to its irregular movements.* Stahl adopted nearly the same opinions, excepting that he substituted the Soul for the Archæus of Van Helmont. Hoffman, inferring that the exciting causes of disease act on the nervous system, concluded, that the effect of this was to produce spasm in the vessels on the surface of the body. Hence the partial arrest of the circulation, the coldness, rigors, etc., during the cold stage. The production of the hot stage is accounted for, by this writer, in the following way. The blood which, by the spasm of the vessels of the skin, has been driven from the exterior to the interior, causes a plethora in the internal organs. This excites the heart, and causes it to repel the fluid back again to the external parts. This action, he concluded, is salutary, since it removes the atony of the vessels, and the congestions dependent on it.† In this way, Hoffman accounted for both the cold and the hot stages. Cullen, adopting the same opinions, considered that the remote cause of fever is of a sedative nature, and that it produces an atony of the skin. This atony, which he ascribed to a diminution of the energy of the brain, produces spasm of the vessels on the surface of the body, and drives the blood to the internal organs. Cullen thus made debility the first link in the morbid chain, and spasm the next. But the debility, he inferred, acts as an indirect stimulant to the sanguineous system. "This stimulant, by the aid of cold, augments the action of the heart, and re-establishes the energy of the brain, and, also, that of the capillaries." Hence the reaction, or the febrile stage. "How," remarks Dr. Cullen, "the state of debility produces some of the symptoms of the cold stage, may, perhaps, be readily explained: but how it produces all of them, I cannot explain, otherwise than by referring the matter to a general law of the animal economy, whereby it happens, that powers, which have a tendency to hurt and destroy the system, often excite such motions as are suited to obviate the effects of the noxious power. I judge so, because the cold stage appears to be universally a means of producing the hot." If so, how are we to account for the production of continued fever, in which there is no cold stage? The writer then adds: "It will still, however, remain a question, what is the cause of this spasm; whether it be directly produced by the remote causes of fever, or if it be only a part of the operation of the *vis medicatrix naturæ*. I am disposed to be of the latter opinion."* Currie has drawn nearly the same conclusions, and has left the question in the same state of uncertainty as before, for the circumstances now referred to are insufficient to account, either for the immediate

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* Works, p. 68.

† The Intermitting Fever of 1661, p. 3.

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* Ort. Med. p. 769.

† Opera. Omnia. vol. 1, p. 167. and vol. 6, p. 165.

* First Lines, pp. 90—93.

cause of ague, or for its periodical return. No attempt was made, after this, to discover the real and efficient, or proximate, cause of fever. In fact, as Dr. Wilson Phillip observes: "From the silence of Physicians on the proximate cause of fever, since the appearance of Dr. Cullen's *First Lines*, and Dr. Brown's *Elementa Medicinæ*, they appear to have regarded it as a hopeless labour."* M. Alibert also remarks: "It is well known, that the attempts, during the past ages, to discover the immediate cause of fevers, have only given birth to obscure theories. It is necessary, as Keil has observed, 'to content ourselves with the historical knowledge of fevers, to study them merely after their signs, their phenomena, and the physical causes which produce them; all the rest is hidden from us,' "† More recently, Dr. Copland, in common with the majority of writers on Tropical Diseases, has referred Ague, and the continued and remittent fevers of intertropical climates, to the operation of malaria on the system, and in the following way. "In these cases, the exciting causes, floating in the air, are received into the lungs; and, if they be concentrated or energetic, they slightly, although they may sensibly, affect the organ of smell in their passage. But their chief action is exerted upon the nerves of the lungs themselves. It may even be admitted, that they, also, partially affect the blood during the digestion of the air, which is their vehicle, by the lungs. Of this however we have no satisfactory proof: and as their direct operation on the nervous influence of this organ is sufficient to produce all the phenomena, it is unnecessary to assign an additional agency to explain them. The morbid impression having been thus principally made in this quarter, it is necessarily extended to those organs, which are chiefly supplied with the same system of nerves; and thus the lungs, the heart, the blood-vessels, the digestive organs, and the secreting and assimilating functions almost immediately experience the effects of this morbid impression. . . . . . The cerebro-spinal manifestations are also early affected—in a slight and passing manner by the impression made by the noxious effluvia on the nerves of smell; but, much more seriously, by the influence exerted by the organic nervous system upon the brain and spinal cord, or extended from the former to the latter, and, consecutively, by the changes in the state of vascular action and of the blood. . . . . . The congestions of the large vessels, and the changes in the quantity and quality of the blood, consequent upon deficient secretion and excretion of its watery and noxious constituents, having reached a certain pitch, *bring about vascular*

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* The Nature of Fever, p. 1.

† Traité des Fievres, p. 216.

reaction, if the organic nerves or vital influence be not too far reduced, or otherwise altered, by the exciting causes."* These conclusions cannot be maintained, for although we might, in this way, account for an attack of ephemeral fever, it would be impossible to explain why the attack should return, after its subsidence, not once only but for months, and at regular and stated periods. The malarious poison could not have remained in the bronchial tubes all this time, not only because it would be liable to be expelled by each act of expiration, but, also, because there is a perfect remission between the attacks, thus showing that the cause, or agent, which produced the cold and febrile stages has been removed to a different situation. Nor would it be possible to account for its re-introduction into the lungs, in sufficient quantity to produce a fresh attack at regular and fixed periods—the quantity of malaria present in the atmosphere not varying, to any appreciable extent, at the *same season* of the year. Besides, persons are sometimes attacked with ague weeks, and even months, after exposure to the exciting cause; and after their removal to a situation in which the same cause is not in operation. When strangers arrive from healthy localities, to visit the sickly districts in the Genesee country, during the hot months, they are seldom attacked, says Dr.

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* Cyclopædia of Practical Medicine, Art. Fever.

Stevens, with the marsh fever in less than a week after their arrival; and, in many instances, where the individuals have remained but a short time, and have then removed to healthy, hilly districts, the poison has been known to remain dormant in the system for several months.* It is also a fact, that some of the officers and men, belonging to the unfortunate Walcheren expedition, were attacked several months after their return to England, with the fever peculiar to that locality, and unknown in England. If, however, ague be due to the direct action of malaria on the animal economy, as was before inferred, where, we may ask, can the poison be situated during an attack? That we will now endeavour to ascertain.

If malaria be diffused generally in the atmosphere of certain localities, as there is abundant evidence to prove,—for we know the circumstances which favor, and those which retard the extrication of this matter from the surface, while the laws regulating its diffusion in the atmosphere are almost as well known as the laws of gravity itself—a certain portion of the poison would enter the lungs of those persons who inhale the contaminated air. As it must, when carried into the bronchial tubes, be brought into contact with a powerful absorbing surface, the natural inference is, that it would pass into the blood with the oxygen of the inspired air.

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* On the Blood, p. 241.

We have proof, in fact, that not only oxygen, but many other gaseous substances, when present in the atmosphere, pass directly into the blood by this channel. Bichat ascertained, long since, that hydrogen is absorbed when introduced into the air passages; and that it communicates its inflammable properties to the living fluid, to such a degree, that the blood, taken from a distant vessel, produced a flame, when a taper was applied to it. Carbonic acid is also absorbed with almost equal rapidity, when inspired by man. Assuming, for the moment, that the malarious poison, when inspired by man, passes directly into the blood,—an inference that will be confirmed by certain facts to be adduced hereafter—the question arises, in what part of the circulating mass is it when the cold stage commences? As the symptoms, then present, are indicative of derangement in the functions of the lungs and of the heart, with partial or total arrest of the circulation, we can hardly avoid concluding that these effects are due to the presence of the poison in the capillaries of the lungs, in the pulmonic vein or in the heart: and its direct action on the nerves which preside over the functions then deranged. It is more likely to be in the former than in the latter situations, for a variety of reasons. As, however, the physiology of the cold stage is somewhat obscure: and as it will be better understood after that of the febrile stage has been discussed, it will be better to pass on to a consideration of the phenomena presented at this period. Apparently, they are the very opposite to those observed during the cold stage. Instead of chills and rigors, there is an increase of heat on the surface of the body; instead of obstruction, there is accelerated circulation, and increased action of the heart. How, then, are these opposite effects to be accounted for? Either the febrile stage is not an effect of the same cause as that which produces the cold stage, or, else, the poison must be in a different situation in the one case to what it is in the other. Is it, then, expelled out of the body by the same channel as that by which it entered; or, is it propelled onward with the circulating fluid into the arterial system? Remarking that symptoms, indicative of derangement in this system, arise immediately on the subsidence of the cold stage, the natural inference is, that the febrile stage is produced by the presence of the morbid matter in the arterial branches, or, in the capillaries, although the phenomena then observed are entirely different to those that occur in the cold stage, for the functions of the systemic capillaries are not identical with those of the pulmonary capillaries. It rests, therefore, to ascertain whether we can, on this supposition, account for the effects then observed.

Much difference of opinion has been expressed respecting the physiological, or pathological, condition of the capillaries in fever and in inflammation—

the conclusions drawn respecting the one being equally applicable to the other. Previously to the discovery of the circulation of the blood, the general opinion was, that the effects observed were caused by obstruction, and the entrance of blood, instead of air, into the arteries. This opinion—that of obstruction—prevailed even after Harvey's immortal discovery, and until the time of Haller and his promulgation of the doctrine of irritability: after which it was concluded, that there is accelerated circulation in the capillaries, not obstruction.* This doctrine has been, with some few exceptions, the prevalent one up to the present time. Nevertheless, it is not a correct one. Such a theory, if sufficient to account for the effects that arise in the febrile stage, will be insufficient to account for those witnessed in the cold stage: it must therefore be rejected, as no theory can be received, that does not account for all the phenomena—those witnessed in the cold as well as in the hot stage. A few writers have advocated the opposite doctrine, that of diminished irritability and circulation. In 1765, Vacca Berlinghieri promulgated the doctrine of relaxation of the capillaries in inflammation, accompanied by diminished action, and stagnation of the blood.[*] Dr. Wilson Phillip has also stated that, in inflammation, the excitability of the capillaries is impaired, as may readily be seen with the assistance of the microscope ; while the difference between inflammation and fever seems only to be, that, in the latter, the debility of the capillaries is general, in the former, it is confined to one part. But he adds, the larger vessels of the inflamed parts are in a state of increased excitement; and the additional stimulus, thus given to the capillaries, appears to be the means which nature uses to remove the disease.[†] A more recent authority—Dr. Copland—infers, that the state of the capillaries is different, according to the type of the disease. " Inflammation, in its more *sthenic* form, is a result of a morbidly excited state of the organic nervous tissue surrounding the extreme vessels, or capillaries of the affected part; or a derangement from an unnaturally exalted condition of those nerves, on which the function of these vessels, and, indeed, of the whole vascular system have been shown to depend." Dr. Copland also considers that, when the organic nerves are thus excited, the diameter of these vessels is enlarged; and that the blood circulates in them with increased velocity. But, in asthenic inflammation, he draws a different conclusion. "In this form, the vital contractility of the capillaries and tissues of the affected part is impaired." And he adds: "there is every reason to infer, that the current of the blood is impeded or retarded in the capillaries at an early stage, or even from the commencement." And again: "The distention of the capillaries in asthenic inflammation is referable to impaired or lost tone, rather than to an active vital expansion, or turgescence, similar to that which characterises the early stages of sthenic inflammations."* According to this doctrine, then, there would be a state of excitement and of increased circulation of the capillary vessels in the sthenic form of fever: and of depression and diminished circulation in the other: in which case, there ought to be two different and opposite causes in operation for the production of the febrile state, but this is not probable. The phenomena, in both instances, are not only similar, but the one form glides so insensibly into the other, that it is sometimes difficult to determine whether a sthenic or an asthenic state exists. These anomalies and contradictions would appear to have received an elucidation from M. Claude Bernard—the Professor of Physiology at the College of France.

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* Glisson was the first to distinctly state, that inflammation was due to irritation in the capillaries, and not to congestion. This theory was adopted by Gorter, one of the successors of Boerhaave, and by Gaubius. "They considered, that this *irritation* increases the action of the vessels, and propels the coloured blood into the colourless capillaries."

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[*] De Inflam. Morb. causis, etc. Flor. 1765.

Opus cit, pages 186 and 188.

This profound Physiologist instituted a series of experiments, in order to ascertain the real functions of the spinal and organic nerves; and he arrived at the following conclusion, among others. "The division of the nerves, belonging to the system of organic life, gives rise: 1st, to an acceleration of the passage of the blood through them; 2ndly, to an increase of temperature; and, 3rdly, to an exaggerated activity of the secretions."* On the contrary, if the nerve be galvanized, the opposite effects are produced—the circulation being restored to its normal condition. As is evident, phenomena, similar, apparently, to those that arise in fever and in inflammation, are thus produced by the division of the organic nerves. We are also certain, that this effect is due to the loss of nervous energy, as galvanism removes the morbid effects, and restores the vessels to their former condition. If, therefore, we allow, that the ganglionic system is in a state of depression, from some particular cause, we shall be enabled to account for the pathognomonic symptoms observed in fever, with two exceptions—the suppression of the secretions, and the fact, that the effects are general, instead of being local. But the organic are not the only nerves distributed to the capillaries and secreting vessels: they are also supplied with branches from the cerebro-spinal system, the influence of which has been demonstrated in the following way by M. Bernard. "When the sympathetic branch of the submaxillary gland is divided, an unusual flow of blood to the part takes place immediately. But the same result is obtained by merely galvanizing the *corda tympani.* These two nerves are, therefore, antagonistic; and the dilatation which occurs in the vessels, when the influence of the organic system has been suppressed, is due to the active interference of the tympanic nerve, liberated from its usual antagonist." (*Loc. cit.* p. 258.) It would thus appear, that the capillary vessels are under the control both of the sympathetic and of the cerebro-spinal systems, and that these nerves are antagonistic. "The existence of two distinct classes of vasomotor nerves," remarks M. Bernard, "is a fact, which now lies beyond the possibility of a doubt." . . . . "The branches of the organic system of nerves are always constrictors, while the dilators proceed from the nerves of the cerebro-spinal axis. In fact, it is always to a nerve of the cerebro-spinal system, that we must, at present, refer the origin of the filaments, which, in the different glands, preside over the phenomena of secretion and of dilatation." (*Loc. cit.* p. 259.) Sir B. Brodie states, that secretion not only requires the vital energy of the sympathetic, but the preservation of the upper portion of the spinal column. According to Sir Benjamin, the medullary source of secretion lies between the *Pons varioli*, and the beginning of the dorsal vertebræ. When this part of the *Medulla Spinalis* had been destroyed, secretion was at an end, although artificial circulation was kept up.* These conclusions granted, we shall, probably, be enabled to offer a satisfactory explanation of the phenomena witnessed during the febrile stage, as, also, in inflammation.

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* Loc. cit., Art. Inflammation.

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* Leçons de Pathologie Expérimentale, p. 265.

If the cerebro-spinal nerves, distributed to the secreting vessels, be in a state of depression, as well as the organic, no matter from what cause, it will not be difficult to understand, why the secretions are suppressed, or diminished and altered, at the very time that the circulation, in the other capillaries, is increased. As there are two sets of capillaries—those which form the connecting link between the arteries and the veins, and which carry on the general circulation; and those destined for secretion and nutrition†—and as a cerebro-spinal nerve would appear to be necessary for secretion, the nerves from this system must be distributed to the secreting, or secerning, vessels, if not to the other capillaries. If, also, these nerves be dilators, or, rather, be distributed to dilator muscles,‡ the secreting vessels would necessarily become constricted by the suppression of their vital influence; provided that there were no other, or antagonistic, power to counteract this effect. Such would be the case, if the ganglionic nerves be distributed to these vessels, the same as to the other capillaries; and if they be, also, in the same state of depression. According to M. Bernard, when both the organic and the cerebro-spinal nerves have been divided, a passive distention of the capillaries takes place, the vessels being thus left entirely to themselves. (*Loc. cit.* p. 258.) But such a state would hardly account for the total suppression of the cuticular secretion in fever. I should therefore infer, although no such conclusion has been drawn by this experimentalist, that cerebro-spinal nerves are alone distributed to the secreting vessels. The object, we may presume, that Nature had in view, was to ensure a constant supply of blood to these vessels: as such, dilators would be principally or alone required If, therefore, the secreting capillaries be provided with dilators only, they would necessarily become constricted, if the vitality or energy of the nerves distributed to them became annihilated or lowered by any cause. "It is always," remarks M. Bernard, "by the interposition of the contractile element, that we can explain the influence of the nervous system: we should, in fact, be unable to admit, that the action of nerves is exerted directly on the blood, to modify either its movements or its chemical composition. In the vessels, the same as everywhere else, it is always on a contractile element, that the nervous influence is exerted." (*Loc. cit.* p. 310.) Hence, the constriction of these vessels in fever, and the suppression of the secretions. According to Dr. Currie, " the capillary vessels, which separate the aqueous part (of the blood), as well as the orifices of the skin that pour it out on the surface, remain constricted, during the whole of the febrile stage." On the other hand, the systemic capillaries, which are, to a great extent, removed from the influence of the heart's action, would require constrictors rather than dilators, in order to propel the blood into the systemic veins. If so, the effect of the depression or annihilation of the vital energy of this class of nerves—the organic—would be the dilatation of the vessels, and the acceleration of the blood in them, together with an increase of heat—the effects observed after the section of these nerves. We shall thus be enabled to account for all the phenomena, or morbid effects, observed in simple and uncomplicated cases of fever, provided only there be a cause in operation, that depresses the energy of the cerebro-spinal nerves distributed to the secreting vessels, as, also, of the ganglionic nerves supplied to the systemic capillaries. That there is such an agent in operation, at these times, we will now endeavour to demonstrate.*

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* Philosophical Transactions, 1811 and 1812.

† M. Fabre, a Professor at Paris, and a disciple of Haller, states that, in the field of the microscope, the blood in the capillaries followed different directions: that it pursued a direct course into the veins, and a retrograde one in the arteries (or, rather, in the other capillaries)—a sort of ebb and flow. *Essais sur differens points de physiologie.* Paris. 1788.

‡ As the dilatation of the capillaries is not a passive operation, "the *active* dilation of these vessels being," as M. Bernard remarks, "one of the best established facts in Physiology," it must be ascribed, like the constriction, to the muscular fibres, now known to be distributed to the coats of the capillaries.

As there can be no doubt, that ague, if not all the other forms of fever, is the effect of the direct action of malaria, we must infer, if the phenomena witnessed in the febrile stage be due to a state of depression of the organic and cerebro-spinal nerves, that this poison acts as a sedative on both systems. No other conclusion can be drawn on the subject. The only question is, in what part of the system is the morbid matter during the febrile stage? It has been before inferred, that the malarious poison is absorbed into the blood, and that it is carried forward, on the termination of the cold stage, into the arterial system. If so, it must ultimately arrive in the systemic capillaries. As, also, there is a tendency to the surface with all extraneous substances, the morbid agent, we may presume, would be propelled into the capillaries on the surface, rather than to the internal vessels; for although inflammation, and arrest of the internal secretions, to a greater or less extent, is generally observed in continued fever, these results are rare in ague; while even in continued fever the poison manifests its influence principally on the external surface. These inferences allowed, we may conclude, that the phenomena witnessed in the febrile stage are produced by the presence of the malarious poison in the systemic capillaries; and its sedative effect on the nerves distributed to the internal lining of these vessels. That this is the case with other poisonous agents, introduced into the blood, has been satisfactorily shown. "The field, or part of the system, in which poisons act, is," says M. Claude Bernard, "confined to the capillary system, to which they are conducted by the arterial current: all poisonous substances, which do not arrive there in a pure, or undecomposed, state, produce no effect."* As regards these morbid agents, this writer has enunciated the following propositions: "(1) The action on the nervous system is effected through the medium of the blood. (2) The poisonous effect is exerted on the periphery of the nerves, not on their central parts." Messrs. Addison and Morgan have also arrived at the same conclusions. They state, "That all poisonous agents produce their specific effects upon the brain, and general system, through the sentient extremities of nerves; and through the sentient extremities of nerves only; and that, when introduced into the current of the circulation, in any way, their effects result from the impression made upon the sensible structure of the blood-vessels, and not from their direct application to the brain itself."*

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* M. Bernard states, that we are able to produce, by lesion of the nervous system, effects similar to those of many morbid states, or diseases. But we cannot produce the specific diseases, such as fever, small pox, scarlatina, measles, etc. And he then asks:—"Shall we, in addition to all we imitate or explain, for ever be compelled to recognize a *special principle*, mysterious in its nature, which we must acknowledge as a morbid or vital phenomenon?" (*Loc. cit.* p. 16.)

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* Leçons sur les effets des substances Toxiques, etc., p. 48.

One phenomenon, not previously considered, requires an explanation. This is, the increase of heat, so characteristic a symptom of fever. Although it was universally supposed, until lately, that the union of the carbon of the blood with the oxygen of the air took place in the lungs, we now know, that this important operation is performed in the systemic capillaries. Although certain that the phenomenon occurs in the extremity of the arterial system, the production or non-production of caloric is still involved in some obscurity. "The only phenomenon which is invariably observed, either when a muscle is galvanized, or when a gland is stimulated, is an acceleration of the circulation, that coincides with a more elevated temperature. Here then we encounter purely mechanical conditions, which attend the dilatation of the vessels. We believe, consequently, without denying altogether the influence of chemical reactions on the production of heat, that they play a less exclusive rôle than has been supposed; and that we must attribute a great part of the phenomenon to the action of causes entirely mechanical and physical."* As regards the increased production, or retention in the body, of caloric in fever, the explanation is not very difficult. In a normal state of the body, a great part of the heat, then produced, unites with the aqueous portion of the blood, and escapes in the form of vapour, or by perspiration. But, when the secretions are suppressed, no vapour is produced, or, if produced, it does not pass out of the system. Hence the increased temperature of the body. "The obstruction of perspiration—of profuse perspiration—in the paroxysm of fever," remarks Dr. Currie, "obstructs the process by which the constitution expels the morbid heat, and thus leaves the system under the influence of a general stimulus of the most powerful nature. This retention of the watery part of the blood, caused by the suppression of the cutaneous secretion, together with the intense thirst, are as remarkable phenomena as any in fever." † Another cause of the phenomenon is the non-production of carbonic acid gas, in its normal quantity: the degree of animal heat appearing to be in an inverse ratio to the quantity of carbonic acid gas formed—red venous blood being, as M. Bernard has remarked, hotter than dark venous blood. This result may be ascribed to the circumstance, that the circulation of blood is increased in the systemic capillaries: it does not remain, therefore, long enough to undergo the usual chemical changes, a certain degree of rest being necessary for the purpose.

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* An Essay on the operation of poisonous Agents on the living Body. London, 1829.

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* M. Claude Bernard, *loc. cit.* p. 278. † Works, vol. 1, p. 243.

Having examined the phenomena that are observed in the capillaries of the skin, it is necessary to inquire into the cause of the increased action of the heart and arteries; or, of the re-action that occurs in the febrile stage. If, as has been inferred, the malarious poison acts as a sedative on the nervous system, the re-action cannot be referred to the direct action of the poison on the heart: instead of increased, there would then be decreased action, or complete arrest, as occurs with other and well known poisonous agents. According to Sir B. Brodie, the infusion of tobacco, when injected into the body in sufficient quantity to produce fatal results, appears to act principally, if not wholly, on the heart. The same may be said of Digitalis and of the Upas Antiar; this organ being found distended after death, paralyzed, and not excitable even by galvanism. According to M. Bernard, these poisons act by destroying muscular contractibility; the nervous system remaining intact and free. The malarious poison, therefore, can exert no influence on the heart, at least during the febrile stage. Its increased action may be thus accounted for. Although, as has been shown, the section of the organic nerves is followed by increased circulation in the capillary vessels supplied by these branches, this acceleration of the blood must be referred to the direct action of the heart, as, otherwise, there would be stagnation. As these vessels, after the operation, lose their contractile power, and become distended, there would necessarily follow an arrest of the circulation, unless for some counteracting force. This is the action of the heart, not only because it is the function of this organ to propel the blood into the extreme divisions of the arterial system; but, also, because we observe similar results, when there is an obstruction to the circulation from other causes. Mr. Blake has shown that, when soda is injected into the axillary artery, the systemic capillaries are so affected (by congestion and obstruction to the circulation) as to require the left ventricle to exert a force, three times greater than it employs in the general state of the circulation, before the blood containing these substances, in any quantity, can pass from the arterial into the venous system.* Hence the cause of the re-action, or of the increased action of the heart and arteries, in the different forms of fever. Accelerated circulation, however, is not a necessary concomitant even of the febrile stage, for in certain fevers—and those of the most severe and malignant type—the pulse is weak, and low, and the circulation languid and oppressed from the commencement to the termination of the attack. The cause of the variation is probably this.

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* On the mode in which various poisonous Agents act on the animal Body.

In consequence of its increased quantity, or from causes with which we are unacquainted, the morbid Agent, instead of being confined in the systemic capillaries is diffused more generally and equally in the blood. As such, the phenomena witnessed in the sthenic form of fever would not be observed: other and different effects would be produced, those met with in asthenic fevers—effects that will be more particularly described hereafter.

The preceding are not the only phenomena observed during an attack of ague: there is, in addition to the cold and the febrile stages, what has been termed, for the want of a more elegant expression, the sweating stage. As this occurs on the termination of the febrile stage, we may refer it to the relaxation of the mouths of the exhalents, or secreting vessels; and the escape of the cuticular secretion, poured out in greater quantity than usual, in consequence of its previous retention. The phenomenon, indeed, admits of a ready explanation; the only circumstance of importance is to ascertain, whether the morbid agent, productive of all the previous effects, escapes out of the system at the same time? That it may escape by the external exhalents, on the termination of the disease, might be inferred; but this does not appear to be the ordinary channel for the expulsion of the poison in ague, as the paroxysm returns, again and again, generally for some weeks, frequently for months, and sometimes for years. To what part of the system, then, is the morbid matter propelled on the termination of the febrile stage? If not out of the body, it must pass, with the current of blood into the venous extremities. But, then, it may be answered, that there are no indications of the presence of a morbid Agent in the system at this period. That may be, for the veins, which perform important functions, and which are merely inert tubes, destined to convey the blood from the systemic capillaries to the heart, are only sparingly supplied with nerves. No appreciable effect, therefore, would be perceived by the presence of a deleterious substance in this situation. It might, if possessed of sedative properties, tend to retard the circulation of blood in these vessels, particularly if they be possessed of muscular fibres, as has been inferred: but this is the only effect likely to be observed. That the poison remains dormant in the system, at this period, may be concluded from the effect of other poisons, which we are certain have been introduced into the blood. Thus, the hydrophobic poison remains dormant, on the average, forty-five days after its introduction: the *minimum*, according to Dr. Gregory, being twenty-one days, and the *maximum nine* months. During all this time, it must have been present in the venous system—the receptacle for all substances introduced, by the absorbents, from without. It is not until it has passed on to other parts of the system, that morbid effects are produced. This has been rendered very evident by the following experiment, performed by Mr. Blake:—"The abdomen of a dog was laid open. A ligature was passed under the vessels entering the liver, and tied. Three drachms of hydrocyanic acid, containing 3·3 per cent. of acid, were introduced into the stomach through an opening made in its parietes. The poison remained in the stomach ten minutes, without producing the slightest general effect. The ligature was then removed, so as to restore the circulation in the viscera. One minute after the removal of the ligature, the animal began to experience the effects of the poison. The ligature was again replaced; but before this could be effected, a sufficient quantity of the poison had been absorbed to arrest the respiratory movements; and the animal must have perished, had not artificial respiration been had recourse to. After this had been continued eight minutes, the animal was sufficiently recovered to continue the respiratory movements itself. After a short interval, the ligature from the *vena portæ* was again removed. The animal was dead in two minutes."—(*Loc. cit.* p. 11.) Majendie has shown, that the action of a poison, injected into a limb, can be suspended, almost indefinitely, by tying the veins above the part in which the poison is situated.

Hence it is, that the fatal effects of the bite of a serpent have sometimes been prevented or arrested by a bandage tied round the limb, and above the wound. That the poison remains in the vein all this time has been conclusively shown by M. Vernière, who states that if, after nux vomica has been introduced into a vein, the circulation in that vein be arrested by a ligature, no effect is produced, although the arterial circulation goes on. If, however, some blood be drawn from that vein, between the wound and the ligature, and if it be introduced into the leg of another animal, without the venous circulation being arrested, the usual effects of poisoning, and even death itself, will follow. We may therefore conclude, that poisons do not produce any sensible effect, while present in the veins: it is not until they have been transported to other parts of the body, or into the arteries, that morbid effects are observed. Thus, sulphuretted hydrogen, which, like the oxide of carbon, has a powerful toxic effect, can be introduced into the system, as M. Claude Bernard remarks, without producing any morbid effect, if it be injected into the jugular or the crural vein of an animal.* The gas passes out of the system by the lungs, as shown by its effect on a piece of paper, saturated with a solution of acetate of lead. But when inhaled, and when introduced into the arterial system, this gas produces speedy and mortal effects. It also appears that the time of action of a poison, after its introduction into a vein, bears an exact ratio with the rapidity of the circulation of the blood, between the vein and the artery. Hence we understand, why poisons act more promptly when injected into the carotid artery or aorta, than when injected into the veins, as shown by some experiments of Mr. Blake.* It has been concluded, from the rapidity with which some poisons act, that they produce their effect on the nervous system, without being absorbed, and without being directly applied to the tissues, the functions of which are disturbed or suspended. "These conclusions have been drawn," says Mr. Blake, "in consequence of the erroneous views which have been taken, of the time required for the blood to circulate from one part of the system to another; and also from the statements of the instantaneous action of the more powerful poisons being deduced from incorrect observations." In order to show with what rapidity some substances are absorbed, and pass from one part of the system to another, the above experimentalist injected a drachm of *liq. ammoniæ*, mixed with 5 drachms of water, into the jugular vein of a dog. "A glass rod, which had been dipped in hydrochloric acid, was held immediately under the nostrils. *Four seconds* after the introduction of the first drop of the solution of ammonia into the veins, it was plainly detected in the expired air by the white vapours that were formed, on its coming into contact with the vapour of the hydrochloric acid." * These conclusions granted, we can understand why there should be complete apyrexia, and absence of all morbid symptoms, in the intervals of the attacks of ague, although a poisonous substance is then present in the system.

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* Leçons sur les effets des substances Toxiques et Médicamenteuses, p. 58.

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* Mémoire sur les effets de diverses substances salines, injectées dans le système circulatoire. Paris, 1839.

We have now to consider the cause of the cold stage, thus ending where, but for the reasons already given, we should have commenced. We shall, in fact, be better enabled now to ascertain the exact situation of the morbid agent at this stage: previously, our conclusions were based on assumptions rather than on positive facts. If, as has been concluded, the morbid agent passes, on the termination of the febrile stage, into the veins; and if no effect is, or can be, produced until it reaches the arterial system; we must infer, that it, or a portion at least, is present in the capillaries of the lungs, when the cold stage commences—an inference that was previously drawn, and which will be confirmed by an analysis of the symptoms. These are all indicative of derangement in the functions of the lungs, with chills and rigors, and arrest of the circulation. The phenomena, therefore, are entirely different to those observed by the presence of the poison in the systemic capillaries. But, then, the capillaries of the lungs are different, anatomically and physiologically, to those of the skin. In the latter, there are two sets of vessels, as already mentioned—the secreting and the continuous, or venous: but, in the lungs, there is only one set, which performs the office of secretion, or excretion, as well as of continuity. The functions being dissimilar, different effects would necessarily arise from the operation of the same cause, or Agent.

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* On the mode in which various poisonous Agents act, etc., etc., pp. 3 and 4.

What the physiological condition of the pulmonary capillaries may be, in the cold stage of Ague, we have no means of ascertaining directly; all conclusions on the subject must therefore be, to a certain extent, hypothetical. If, however, these vessels be supplied with constrictor, as well as vaso-dilator nerves; and if the malarious poison acts on them as a sedative, the result would be a state of congestion; the section of both sets of nerves, according to the experiments of M. Bernard, as previously mentioned, producing a passive distention of the vessels. But the symptoms, then present, denote contraction not dilatation of the capillaries of the lungs. How, then, is this anomaly to be explained? It is accounted for in the following way by M. Bernard. "Fever appears to be a peculiar state of the economy, in which the vaso-motor nerves undergo a temporary paralysis: from which arises an unusual activity of the vascular phenomena. But these effects are preceded by the very opposite state, as is evidenced by the chills which precede the apparition of the fever; a contraction of the capillaries would appear to be produced at first, then a dilatation.* . . . An analogous phenomenon is produced on the external surface, after a sudden immersion in cold water; the vessels contract at first, and the general temperature is lowered for some seconds. But, immediately after, the capillaries dilate, and are filled again: the blood flows to the skin, and the temperature rises: it is a phenomenon described by the Hydropathists under the name of *re-action*."† Although the preceding explanation may be sufficient to account for the slight chills, which precede an attack of continued or remittent fever, I can hardly believe, that the capillaries of the lungs are in a state, first of contraction, and then of distention, during the cold stage of ague. When the contraction ceased, the chills and shivering would cease also. But these phenomena remain the same, or nearly the same, during the whole period of the cold stage, which continues, sometimes, several hours.

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* M. Bernard is not referring here to intermittent fever, but to the consecutive fever which arises after local injury—a distinction of some importance.

† Leçons de Pathologie Expérimentale, p. 228. 1872.

The cold chills and shiverings, observed in this stage, may be referred, in all probability, to the arrest of the circulation in the systemic capillaries—the same result, or trembling, being produced in a muscle, when the blood is prevented entering by means of a ligature applied to the artery. The analogy is still more perfect, when animals have been poisoned by nicotine, general convulsions being produced when the dose is sufficiently large. As the action of nicotine appears to be exerted on the nervous and vascular systems—the capillaries being found contracted, and entirely empty, after death—the same effects may be produced by other Agents; and particularly by those that depress the vital energy of the nerves, either of the organic, or of the cerebro-spinal system. I am, therefore, induced to infer, in the absence of all direct proof on the subject, that the capillaries of the lungs are in a state of contraction until the termination of the cold stage, and for other reasons. In the 1st place, it is probable, that the pulmonary capillaries, like the secreting vessels, are not supplied with constrictor nerves and muscles; the circulation, in these vessels, can be readily carried on without their aid, and by other, if not more powerful, means. Unlike the systemic capillaries, the distance between them and the heart is so short, that the force of this organ alone would, we may presume, be sufficient to drive the blood through these vessels and into the pulmonary vein.

The action of the muscles of the thorax, during the act of expiration, would also tend to produce the same result. In the next place, these vessels, although connecting tubes between the pulmonary artery and vein, perform another important function, that of excretion and absorption. As such, it is of more importance that they should be in a state of distention than of contraction, otherwise these functions could not go on regularly and constantly, as is absolutely necessary for the preservation of health and of life. If therefore we conclude, that the capillaries of the lungs are only supplied with cerebro-spinal, or dilator, nerves; and if we also conclude, that the malarious poison is present in these vessels, during the cold stage of ague; the same effect would follow as in the secreting vessels of the skin during the febrile stage: viz. constriction of their coats, and, with it, an arrest of the circulation more or less complete. If the pulmonary circulation be arrested, there must also be stagnation of the general circulation to the same extent, or degree. Hence the symptoms observed in this stage of the disease: the cold breath, the difficult breathing; the failure of the pulse; the wrinkled, colourless, or livid, state of the skin, and of the mucous membrane of the mouth, lips, etc. This state will continue until, by the efforts of Art or of Nature, more generally the latter, the morbid Agent has been expelled, either out of the system, or, else, has been driven into the pulmonic vein. It cannot be the former, for the cold stage is invariably or almost invariably followed by the febrile stage: it must therefore be the latter, and hence the return of the febrile stage, as well as of the cold stage.

In some rare cases, and in particular localities, as the South of Europe, a different order is observed. Instead of the cold stage being followed by the hot stage, vomiting and purging supervene; the arrest of the circulation becomes complete, and the patient falls into a state of confirmed collapse. This forms what is termed malignant Ague by English Authors, *febre perniciosa* by the Italians, and *fièvre pernicieuse* by the French. As the phenomena are exactly similar to those presented during the collapsed stage of cholera;—so similar, indeed, that the first cases of Asiatic cholera, in the South of Spain, were considered by the resident Practitioners there to be cases of Ague; while the first case of malignant Ague, that I saw in this country, was taken by me to be one of Asiatic cholera—and as the Physiology of this disease will be discussed hereafter, it is unnecessary to enter into this part of the subject now.*

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* In the 1st Edition of my Work on the Epidemic Cholera, a chapter was devoted to the Physiology of this disease, as deduced from that of Intermittent Fever. It was omitted in the subsequent editions, thinking it best to wait until the Physiology of Intermittent fever had been more fully and satisfactorily cleared up, the two subjects being so intimately connected together.

The result of the preceding arguments and conclusions, respecting the Physiology of Intermittent Fever, may be thus summed up.

(1.) The remote cause of intermittent fever is the presence, in the atmosphere, of a specific poison to which the term malaria is usually applied.

(2.) The immediate, or proximate, cause is the presence of the morbid agent in the blood.

(3.) The accumulation of the poison takes place in the venous system, being introduced slowly and gradually into the lungs with the air inspired.

(4.) In the cold stage of the disease, the matter is situated in the capillaries of the lungs, producing, by its presence there, spasm, or contraction, of these vessels, with arrest of the circulation to a greater or less extent.

(5.) In the febrile stage, the same matter is confined in the systemic capillaries, having been transported there, with the current of blood, on the termination of the cold fit.

(6.) Although the poison, during the above periods, is contained in the blood, the effects previously described are not produced by any alteration in the properties of this vital fluid: they are due, solely and entirely, to the toxic effect of the morbid matter on the nervous system.*

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* Bichat considered, that diseases were caused by a lesion of the *animal sensibility*, and of the organic contractility—that the cause, in fact, of the phenomena was in the solids : *not* in the fluids. But

(7.) The poison, during the intermission, is contained in the venous system, into which it has been propelled on the termination of the febrile stage.

(8.) Lastly, it is to this circumstance—the retention of the poison in the venous system, for a certain time, until another paroxysm has been produced,—to which we must ascribe the Periodicity of Intermittent fever.

Having thus attempted the solution of a problem, that has remained unsolved up to the present time, it only remains to offer a few remarks on the cause of the variations observed in intermittent fever, which presents itself under three different forms—the Quotidian, the Tertian and the Quartan, the only regular forms observed: the others being irregular, and merely combinations of these three. Of these, the tertian may be considered, not only as the most regular, but as the primitive form: quotidians and quartans changing frequently into tertians, and the latter into quotidians and quartans. The tertian

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he added: "Do not imagine that the fluids are as nothing in diseases, they are the vehicle for the morbific matter . . . . It is manifest, that we must well distinguish diseases themselves, or, rather, the totality of the symptoms which characterize them, from the causes which produce them, or keep them up. Nearly all the symptoms may be referred to the solids : but the cause may be in the fluids, as well as in the former." He summed up his ideas in this Axiom: "Every exclusive theory, either of solidism or of humorism, is a pathological anomaly : as a theory, which ascribed everything to the fluids or to the solids, would be a physiological one."

may be considered the mildest form: the quotidian less so, and the quartan the most severe, and the most rebellious to treatment, while it frequently assumes the malignant form. Like the periodicity, no satisfactory explanation has been given of the cause of these variations. As the symptoms, in one form, are less intense than in the others, while, also, it is more amenable to treatment, we might be disposed to conclude, that the difference of form is to be ascribed principally, if not entirely, to a variation in the quantity of poison present in the system. Although quartans are more apt to prevail in the Autumn, when the quantity of poison present in the atmosphere is the greatest, still, as all three forms are found to prevail at one and the same time, we must look to internal, not external, circumstances for the cause of the variation. If we examine the Individuals suffering from the different forms of fever, we shall find, that Tertians affect principally those who are in a comparatively sound state of health; while those of a bilio-sanguineous temperament are generally attacked with the quotidian form. Quartans, on the other hand, attack those more particularly of a weak, leuco-phlegmatic, habit of body, or who have suffered previously from attacks of other forms of ague. In these cases, there will frequently be found a morbid state of the liver and spleen, with congestion of the abdominal vessels, to a greater or less extent. These circumstances may enable us to account for the variation in form presented by intermittent fever. As the circulation, with the last class of persons, is generally languid, the poison would necessarily be longer traversing the venous part of the system, and hence the length of the remission. The contrary would be the case with those of a bilio-sanguineous temperament, and hence the shortness of the intermission, and the quotidian form of fever. With those, on the other hand, who partook neither of the leuco-phlegmatic nor of the bilio-sanguineous temperament—the circulation being more rapid than in the one, and less rapid than in the other—the Tertian form would prevail. Hence, as I should infer, the variation in type of intermittent fever.

One other subject remains to be considered, before closing this part of the subject: the variations observed in Continued and Remittent fevers. Referring to the former, Dr. Copland remarks: " It would seem that a marked tendency to periodicity exists in all diseases, and that the continued type is imposed—1st, by a high degree of inflammatory action; and 2ndly, by impeded or interrupted secretion or excretion, and consequent alteration of the quality and quantity of the circulating fluid.* And it is added, in another place, " As soon as the blood becomes affected, complete remissions are never detected." The fallacy of these conclusions must be self-evident. Continued fever presents its peculiar form from *the commencement;* it does not begin as intermittent fever and end as continued, excepting in rare instances. The type, therefore, cannot be referred to suppressed secretions and contamination of the blood; for such a cause could not have been in operation a sufficient time to produce the effect under consideration. As to the inflammatory action that may be present, this must be referred to the same cause as that which produces the febrile symptoms, being sometimes present, and sometimes absent: while it more frequently follows than precedes the attack of fever. It is, therefore, a common effect of a common cause. What this cause is, we will now proceed to consider.

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* Opus cit. parag. 157.

As the quantity of malaria present in the atmosphere, in those localities, and at those seasons, in which intermittents prevail, is comparatively small, it will be imbibed into the system in infinitesimal doses. No effect, therefore, can be produced during its passage through the arterial system: it is only when it has accumulated, to a certain extent, in the venous system, and been propelled on to the lungs, that its presence becomes manifest, by the supervention of the cold stage. But in other situations, and at other times—as in intertropical regions, or in the hot seasons of temperate ones—when the quantity of malaria, present in the atmosphere, is much greater, the matter will necessarily be imbibed to a proportionate extent, and with more rapidity. Morbid effects are, therefore, likely to occur, during the passage of the poison through the systemic capillaries; the same as occurs in the febrile stage of intermittent fever, after the accumulation of the morbid agent in the system. The atmospheric temperature also being higher, there would, of course, be a greater tendency to the surface than under other circumstances: the malarious poison, therefore, instead of passing into the venous extremities, would be propelled into the external capillaries, or secreting vessels. As, also, it is precisely these vessels, as was before inferred, that are principally constricted during the febrile stage, the morbid matter would be retained there until, by the efforts of art or of nature, it had been expelled from the system. Hence the longer continuance of the febrile stage in continued fever, and its termination in recovery or death. But the external capillaries are not the only ones, that are affected in these cases; the severe forms of continued fever being generally accompanied by internal inflammation—the effect of the retention of the same poison in the internal capillaries. This result, so seldom observed in intermittent fever, must be referred entirely to the greater quantity of morbid matter in the system, and its propulsion to the internal, as well as to the external capillaries. These circumstances, if allowed, will account for the variation that occurs between continued and intermittent fevers.

In Remittent fever, an intermediate state exists, the remission, although regular, being incomplete. In this case, we may infer, that a portion of the poison passes into the venous system, when the remission occurs, while the greater part remains shut up in the secerning vessels. Hence, there will be a continuance of the febrile stage, but accompanied by slight and partial remissions. As, also, the quantity of morbid matter, passing into the venous extremitics, is small, as proved by the slightness of the remission, no effect will be produced during its passage through the lungs. The cold stage, consequently, will be wanting.

The same arguments and conclusions will hold good with respect to inflammation, which is evidently produced by the same cause as fever, for the phenomena in each are identical. This inference has been confirmed by actual experiment. M. Bernard tells us that, when rabbits are placed under total abstinence, they generally live a fortnight or three weeks; but, when certain branches of the sympathetic nerve have been previously divided, the animals die within a few days, through acute inflammation of the viscera connected with the nervous twigs that have been divided. (*Loc. cit.* p. 31.) Inflammation, therefore, like fever, must be ascribed to loss of vitality, or depression, of the nerves diſtributed to the vessels of the part affected; not to an inflamed or morbid state of the blood, and the consequent irritation of the internal coats of the vessels. The depression, in this instance, like that in fever, must be caused, excepting in cases of external injury, by the presence of a poison in the blood: and, as we are unacquainted with any other morbid agent—the operation of which is universal—excepting malaria, we may refer all specific inflammations to this single cause. This will evidently be the case, when the inflammation is accompanied by a specific form of fever, either intermittent, remittent, or continued; and, as it is an axiom in physics, that like effects are produced by like causes, we may draw the same conclusion with respect to all other inflammations, the cause of which is unknown.

Such are the conclusions at which we must arrive by induction—by an analysis of the phenomena presented to our notice in attacks of fever and of inflammation. It is not sufficient, however, to adopt this method of reasoning in all cases, particularly when we can pursue the opposite one—that of Synthesis or experiment. The inductive method of Bacon is the best that can be adopted in the purely abstract sciences: but that of experiment, or counter-proof, invented by Galileo and Newton, is the only one to be depended on in physiological inquiries. If the effects observed in the different stages of fever be due to the presence of a poison in the system, these effects ought to cease, if the morbid matter be expelled from the body by any means, or if it be neutralized, while present in the blood, by the administration of an antidote. It is desirable, therefore, to ascertain, whether such a result has been obtained—a part of the subject that will require a separate consideration.

CHAPTER II.

RATIONALE OF TREATMENT.

If, as was previously inferred, diseases be produced by the operation of a poison in the system, there are only two methods of treatment that can be scientifically adopted: one is, to neutralize the offending matter by the administration of an Antidote, or, failing this, to cause its expulsion by the employment of agents adapted to the purpose. This is the method invariably adopted, when poisonous Agents have been taken into the stomach, either by accident or by design: and this is the course that was usually pursued by the Fathers of the medical art. Sydenham, that true disciple of Hippocrates, states, that the curative indications are two-fold: 1st, From a careful observance of the steps which Nature takes to overcome the disease, to quicken the fermentation already raised, and thus aid in expelling the morbid matter, and restoring the patient. 2ndly, To endeavour to investigate the specific cause, in order to remove the disease by effectual and specific remedies. Not being acquainted with any specific remedy for fever, Sydenham treated this disease, and all other specific diseases, on the first named principle,—that adopted by Hippocrates. It was an Axiom of the Father of Medicine that *Nature cures diseases.*[*] Galen also tells us, that it is the office of Nature, which formed the body at first, to restore it again to health, when it becomes diseased.[†] The way in which Nature cures diseases is by means of *Crises*, or critical evacuations: and these are produced, either by an increase of the natural secretions, or, else, by some unusual discharge, as already pointed out in the Introductory Chapter. In Fever, sudden and profuse perspiration is the most common means by which Nature attempts to cure the disease. That slight bronchial affections are frequently cured by a spontaneous expectoration, is familiar to all; but fevers do not terminate in this way, unless the fever be sympathetic of an affection of the chest. Although somewhat rare, they sometimes pass off by salivation. Sydenham states, that the continued fever of 1669 was frequently cured by a *spontaneous* salivation. The same fact has been notified by other Writers. A fever may also be cut short by spontaneous vomiting, if it occur at the commencement of the attack. As a copious sediment in the urine is usually a sign, that a crisis has arrived, we may conclude, that morbid substances in the blood pass off spontaneously by this channel, the same as by others. In other instances, Nature attempts to get rid of the disease in another way, by some abnormal discharge; and the most common of any is hæmorrhage. The formation of abscesses is the ordinary termination of many fevers; hence, in Plague, a bubo was always regarded as critical, and its formation encouraged by the Physicians. Boils, also, are salutary, and critical in many fevers.

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[*] De morbis vulgaribus, L. 6. S. 5.

[†] Hippocratis Commentarii.

These crises, it may be remarked, are apt to occur on particular days, or at a certain period after the commencement of the attack. If we regard the days, that have been considered as more particularly *critical*, we shall find that they correspond with the periodical returns of ague. Thus, the critical days are either the 2nd, 3rd, 4th and 5th, corresponding to the quotidian paroxysms; or, the 3rd, 5th, 7th, 9th and 11th, corresponding to the Tertian periods; or, else, the 4th, 7th, 10th, 13th and 16th, which would be the quartan. Salutary crises may occur after this, but they are then rare. The only way in which we can account for the occurrence of crises, on particular days, is by supposing, that there is a tendency to intermission in all fevers; so that the portion of morbid matter which, on these particular days, would have passed into the venous extremities, is expelled out of the system by the skin or other channel. Be this as it may, we can only refer the beneficial effect of these crises to the expulsion of some extraneous substance, the cause of all the morbid phenomēna. That it is not due to the mere increase of the secretions; to the discharge of so much matter; the loss of so much blood; or the metastasis of the disease from one part of the system to the other, by the formation of an abscess, etc., etc., is shown by the fact that, in other cases, and at other times, these effects, instead of being salutary, are injurious—sometimes fatal. As Hippocrates has remarked, Nature acts without reflection or design; hence she sometimes promotes secretions that are either improper, or which are insufficient to expel the *materies morbi* out of the system. Although hæmorrhage from the nose is generally salutary, while it is indicative of a tendency of the morbid matter to the surface; that from the lungs or the bowels is as generally injurious, on account of the difficulty of arresting the flow of blood from these organs. It also shows, that there has been a transference of the morbid matter from the external surface to the internal and vital organs, otherwise these effects would not have been observed. Evacuations from the bowels, which are critical and salutary in general, particularly when they are feculent, and when they occur at an early period of the attack, are bad signs at other times,—when they occur at a late period of the attack, and are serous, instead of being feculent. We are told by Sydenham that, in the fevers which prevailed from 1661 to 1664, diarrhœa was apt to occur towards its termination, "whence, unless the Physician, by chance, should stop it at the commencement, the disease was prolonged, and rendered more obstinate."* Still more dangerous and fatal results are observed in cases of malignant ague, when, in consequence of the sudden transference of the poison to the internal and abdominal organs, and of the complete arrest of the circulation in the pulmonic capillaries, vomiting, purging, and collapse, supervene—the evacuations being entirely serous. Although an increase in the perspiration, more especially its return after it has been suppressed, is almost always favorable in fever, it may, in other instances, be either useless or injurious. Such is the case in ague, the profuse perspiration, that occurs in the sweating stage, not being attended with any beneficial effect, and for the reasons previously given. It is injurious also when the discharge, instead of being a secretion from the arteries, is rather an exudation from the veins; as is observed in the collapsed stage of cholera, and in malignant attacks of ague. It is a sign of the complete stagnation of the circulation, and a most fatal symptom.

It is thus evident, that Nature, although sufficient for the cure of diseases in some cases, is insufficient or detrimental in others—instead of curing, she actually kills the patient. The efforts of Art are therefore required, in order to aid, regulate, control, and, sometimes, counteract, those of Nature—the first lady Doctor. Although so great an Admirer, and so true a Disciple, of this renowned doctress, Hippocrates remarks:—"Not only were we to assist Nature, but to restrain her motions, or direct her when she is wrong." And he adds:—"If the humours tend to an improper part, we should make a revulsion of them from that part."* We shall find, however, on inquiry, that not only the Fathers of the medical art, but those of their successors, who have been the most successful in the treatment of diseases, have taken Nature as their guide. Even those Practitioners, who have repudiated Nature and her teachings—and their name is legion—following other guides, have been obliged to employ Agents, although adopted for other and different reasons, that produce the very effect she has in view, in the cure of diseases. "The Physician," as one Writer has remarked, "whatever his pretensions may be, and whatever his knowledge and acquirements may be, is merely the servant of Nature; she is the Physician, and he the Apothecary, no matter what high sounding Title he may affix to his name; she orders, and he obeys: she directs, and he follows her instructions; she prescribes the remedies, and he administers them. He has frequently, in the pride of his heart, and in the presumption of mental acquirements, attempted to throw off this yoke, and to become the Master, instead of being the Slave, to this imperious Mistress. But every attempt of this kind has hitherto failed, and the result has been disgrace to the innovator, and injury or death to those committed to his charge." In fact, the *modus operandi* of the majority of remedies, that have been successfully employed in the treatment of diseases, up to the present time, can only be explained on the supposition that they expel some morbid matter out of the system. "Our Fevers," says Sir John Floyer, "have all the symptoms described by Hippocrates, and are cured by the same evacuants."* Experience has taught practitioners, that they could not cure diseases by any other remedies; and they have continued to be adopted, without any reference to their *modus operandi*, and, frequently, in direct opposition to the prevalent opinions of the day. That the majority of remedial agents act in the way now mentioned, it will not be difficult to show.

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* Opera, p. 39.

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* Epidemics. Book 6. Sect. 2.

Although we know very little, at present, of the manner in which remedies act in the system, we are well acquainted with their ultimate effects. Bichat states, that "it is extremely difficult to class remedies according to their *modus operandi:* but, certainly, it is incontestable, that all tend to restore the vital powers to their normal type, from which they were separated by diseases. Since the morbific phenomena are all intimately comprehended in different alterations of these forces, the action of remedies ought evidently to consist in restoring these alterations to their natural type." But this celebrated Professor had no idea of the mode in which this was effected. "The action of remedies," observes M. Bernard, "cannot be explained on chemical or on physical principles; only on physiological ones." And he adds:—"There is, in each organ, an *elective* action, which renders it more apt than the others to be influenced by certain medicinal Agents. Hence medicines, which are introduced into the economy, exercise, on the organs which serve for their elimination, an action altogether specific." It matters not, in a practical point of view, whether the election rests with the remedy or with the organ: it is sufficient to know, that nearly all medicinal substances exert a specific action on some particular organ or part of the body. This is not to be wondered at when we remember, that the majority of remedies are poisons, and that there is a tendency in the system—a *vis medicatrix naturæ*—to expel morbid and extraneous substances out of the system, by some particular channel. It may, in truth, be laid down as a "Law" of the animal economy: and hence the division of medicinal substances into Purgatives, Emetics, Diaphoretics, etc., in accordance with their action on particular organs. As these substances will necessarily produce a certain effect on the organ through which they pass, the result will be, when that organ is a secreting one, an increase of its secretion: and hence the beneficial result observed. But other remedial agents are employed, the effect of which is different; their action being confined to the animal fibre or the nervous system—the energy of which they increase. These remedies act indirectly, not directly, and tend to produce the same ultimate result, as we shall presently find. For convenience sake, these remedies may be divided into four classes, according to their *modus operandi.* In the first, may be placed blood-letting; in the next, evacuants—as emetics and purgatives; in the third, diaphoretics, diuretics and sialogogues; and, in the fourth, tonics and stimulants. The effect of these, in the treatment of fevers, we will now proceed to consider.

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* Commentaries on Hippocrates, p. 40.

And, first, as regards blood-letting. When resorted to at the commencement of an attack, or in the formative stage, venesection would be useful by relieving the congestion of the venous system, which, as was before inferred, always exists at this period. If, also, a poison be present in the blood, and if it exist in greater quantity in the venous system at this particular period, as it was my object to show, in the preceding chapter, is the case, a certain portion of the morbid matter would necessarily be abstracted at the same time. We can thus understand why venesection, when resorted to at this period, has been attended with beneficial results, in the majority of fevers, as, also, in the cold stage of ague. It was employed in the last named disease, and its efficacy vaunted, by Dr. Macintosh; by Ali, an Arabian Physician, surnamed the Wise, who lived in the 10th century, and by many other Writers. As, however, every measure that lowers the system is injurious in ague, if the operation failed to remove the greater part of the poison out of the system, it was productive of serious, sometimes dangerous results. Sydenham states that, in the tertian intermittents, which prevailed from 1661 to 1664, "if the bleeding did not produce an immediate cure, it seemed to prolong the attack, even in the strong and robust; while, with the aged, it caused a fatal termination."* The operation has also proved successful, even in the purely adynamic forms of fever, when resorted to at the commencement of the attack—an example of which has been already afforded in the Introductory Chapter. As the cause of the loss of vital power, in these cases, is the presence of a poison in the system, if a sufficient quantity of the matter be removed by the operation, the effect would be, not the lowering, but the raising, of the vital energy.

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* Opera, p. 80.

Hence the beneficial effect of venesection in such cases.

In the febrile stage, the operation is less indicated than in the previous one, but it is generally attended with beneficial results in the sthenic form of continued fever. Although it has been inferred, that the *materies morbi* is principally contained in the capillaries of the skin, at this period, the abstraction of blood from the arm may yet be the means of expelling some of the poison from the system. The effect of such an operation, by relieving the state of congestion that exists, both in the venous and in the arterial systems, would be to relax the secreting vessels: copious perspiration, therefore, as well as alvine evacuations, frequently follow the abstraction of blood. As a certain portion of the febrile matter, if present in the external capillaries, would necessarily escape at the same time, we can understand why venesection, if resorted to at the commencement of the febrile stage, has frequently cut short the attack.

Instead of being beneficial, venesection is injurious in numerous cases—sometimes fatal. The operation may be beneficial at late periods of the attack, in the purely sthenic forms of fever, but such instances are rare. It was a general rule with Hippocrates, and the ancient Physicians, never to bleed after the *fourth* day, in acute diseases: such as Fevers, Pleurisy, Pneumonia, Inflammation of the

Brain, &c. In one particular case, referred to by Hippocrates, it is stated, that the patient was bled on the eighth day. Galen contends, that this fact was mentioned, because it was contrary to the usual rule: which was not to bleed after the *fourth* day. In the purely adynamic forms of fever, the abstraction of even a small quantity of blood, after the febrile stage has been fully developed, or after the disease has existed some days, would be attended with hazardous, if not dangerous results. Even M. Broussais remarks: "When the inflammation of Typhus is not attacked at its commencement, the abstraction of blood is often dangerous." Instead of favouring the elimination of the poison, the operation, by lowering still more the energy of the nervous system, would be the means of preventing it. Sydenham considered that bleeding, when carried too far, or when improperly performed, was not only injurious, *per se*, by the debility it produced, but that it prevented the separation of the morbific matter, present in the blood. "For truly, if I should order bleeding for these (the cases referred to), the blood, already too weak, even without the loss of it in this way, would be rendered altogether unfit for the despumation it has to go through, whence a corruption of the whole mass of blood might follow; and the death of the patient himself, perhaps."* We can thus understand why venesection should be beneficial at one time, and injurious at another. We also find, that these results are to be ascribed, not to the lessening of excitement on the one hand, or to the production of debility on the other; but, to the action of this remedial agent in favoring or retarding the propulsion of a morbid matter out of the system. This was Sydenham's opinion. "The true and proper indications, which present themselves in this disease, as I consider, consist in this: that the commotion of the blood should be restrained, after the example of nature, and suitable to the end in view: for which reason, it ought not to rise too high on the one hand, whence dangerous symptoms might follow; nor, on the other, to become too languid, whereby, either the expulsion of the morbific matter may be prevented, or, the endeavour of the blood attempting a new condition would be weakened (frustrated)."* (*Loc. cit.* p. 41.)

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* Opera, p. 42.

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* The almost total disuse of bleeding, in the present day, has been ascribed, by some Writers, to a change of ideas; or to an improved and more scientific method of treatment. This is an error: it must be ascribed entirely to a change in the *type* of diseases, a change that is contemporaneous with the advent of the epidemic cholera. You do not now meet with the class of fevers that prevailed, when I first entered the profession : nearly all of a sthenic form, and characterized by a full, bounding, pulse, high fever, and, when not cut short, attended by acute inflammation of the brain and other organs. Not to have bled in such cases would have been to consign the patient to certain destruction : the same as it would be to bleed to the same extent in the low, adynamic, fevers, that prevail in the present day. When a pupil of the late Sir Richard

We may now turn to the class of Evacuants. And first as to Emetics. These remedies were generally resorted to, by the older Physicians, in all cases of fever; but, in consequence of the promulgation of modern theories, more particularly that which ascribes fever to inflammation of the brain and intestinal canal, they have now fallen into entire disuse. They are indicated in the formative stage of continued fever, and shortly before the commencement of the cold stage of Ague; at which periods they were employed by the Greek and Arabian Physicians, and by nearly all the followers of Hippocrates; as, also, by other Practitioners, although their opinions were different to the former. It was the practice of Sydenham, in the continued fevers of his time, to administer an emetic at the commencement of the attack, unless contra-indicated by the tender age or weakness of the patient. This rule was observed by him more rigidly, when vomiting had occurred previously and spontaneously: and he states, that if emetics, when thus indicated, were omitted, Diarrhœa, difficult of cure and dangerous to the patient, was sure to supervene. "It is probable," observes Cullen, "that the vomit-

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Dobson, at the Royal Marine Infirmary, Chatham, I have sometimes bled 4 and 5 patients at a time, on their entrance: but it was very rare for us to lose a patient from fever at that time. The disease was usually cut short by the treatment. The patients, it is true, were all men in the prime of life—soldiers.

ing, which occurs so frequently in the cold stage of fevers, is one of the means employed by nature for restoring the determination to the surface of the body:" and he adds, in proof of this view, "that emetics, thrown into the stomach, and operating there, in the time of the cold stage, commonly put an end to it, and bring on the hot stage."

The *modus operandi* of this class of remedies is easily understood. By the strong contraction of the muscles of the abdomen and chest, a powerful effect is produced on the whole of the internal organs, and, consequently, on the abdominal and other veins. By relaxing the vessels on the intestinal canal, some portion of the poison which, it has been inferred, is principally situated in the great venous trunks, at this period, could hardly fail to be expelled. Hence their beneficial effect.* Independently of this result, the excretory ducts of the fauces, of the liver, and of the intestines, are powerfully excited, and their secretions increased, during the operation of an emetic. The skin, also, is similarly excited, and profuse sweats are often produced. Emetics might thus be beneficial, not only in the formative stage, but, also, in the febrile stage of continued and other fevers, even supposing that the greater part of the morbid matter is contained in the capillaries of the skin at this period. Keil, Cheyne, and several other Writers, who advocate their employment in Typhus fever, state, that they have successfully employed them even at late periods of this disease.

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* Ali, the Arabian Physician, recommended Emetics as prophylactics in fever; or, against that state of the humours, which, as he inferred, was productive of the disease.

In other cases, the employment of emetics, so far from being beneficial, is actually injurious. This will be the case, if employed during the height of the febrile stage, when the vascular excitement is high, with determination of blood to the head. Not only would the latter effect be increased, during the operation of an emetic, but the administration of such a remedy, when the fever runs high, does not usually produce the same effect as in other cases, or relax the vessels on the surface. As such, the employment of emetics, at this period, would not tend to effect the principal object we have, or ought to have, in view, during the treatment of fevers—the expulsion of the poison out of the system. On the contrary, the morbid matter, by the nausea and exhaustion subsequently produced, might be determined from the external surface to internal and more vital organs. For these reasons, Sydenham generally employed blood-letting, previously to the administration of emetics in continued fever. They would also be injurious, when resorted to at a late period, particularly in adynamic forms of fever, by increasing the debility and exhaustion experienced in these particular states, and favoring, in consequence, the determination of blood, and, with it, the poison itself, to the internal organs. The administration of an emetic is also contraindicated after the cold stage of ague has fairly set in. Instead of favoring the propulsion of the morbid matter from the lungs to the external surface, it might cause a revulsion to the abdominal organs; and thus, not only keep up the vomiting, but be productive, at the same time, of serous evacuations,—the same as is observed in malignant attacks of ague.

The next remedies belonging to this class, that we have to consider, are purgatives. They were much resorted to by the ancient Physicians, and they have continued to be employed, to a greater extent, perhaps, than any other remedy, from that time to the present. Purgatives were generally resorted to in Typhus fever, during the past two centuries; and they still continue to be employed, in this disease, in England, although not so freely or so generally as before. On the Continent, they were always more sparingly employed; while they have been almost entirely banished from practice since the adoption of the doctrine of Broussais. These remedies are more particularly indicated, as will be apparent, during the remission of intermittents, and in the premonitory stage of continued and remittent fevers. When administered at these periods, they would, by increasing the excretion from the large intestines, favour the expulsion of the poison, then present in the venous system, by that natural outlet for the elimination of extraneous substances in the blood. Of the class of purgatives, Calomel is, perhaps, the best in ague. Hildebrand mentions a case in which it succeeded, after all other remedies had failed. It has also been largely employed in India by Lind, Johnson, Annesley, and others. I have also administered it, with great success, in those cases of fever attended by enlargement of the Liver and Spleen, so frequently met with in Spain and in Italy. To increase the effect, and quicken its operation, the Calomel should be combined with aloes or colocynth, not with saline purgatives. Although to a less extent than emetics, purgatives produce an increase in all the other secretions, even in that of the skin. They will therefore tend to expel any morbid matter, that may be present in the external capillaries; and hence their beneficial effect, not only in the premonitory, but, also, in the febrile stage of continued and remittent fevers. Not being attended with the same mechanical effects as Emetics, they can be employed in cases in which the administration of the latter would be dangerous. Hippocrates, even in those cases in which these remedies were indicated, did not purge until after bleeding. This has invariably been found to be the best method, whenever such an operation is indicated, or rendered necessary; purgatives acting more readily, and more beneficially, after the state of venous congestion has been removed. They would also act more freely on the secretions and excretions, after the removal of a portion of the poison—the cause of their suppression. In Typhus, and other adynamic forms of fever, when, as we may infer, a considerable part of the poison is contained in the internal organs, the beneficial effect of purgatives is readily understood.

But these medicinal Agents, like the preceding, are sometimes injurious, and particularly in the low, adynamic, forms of fever. Instead of favouring the propulsion of the morbid matter out of the system, their employment, in such cases, might favour, still more, its determination to the internal and vital organs—the principal cause of the dangerous symptoms in these forms of fever. A similar result may follow the administration of purgatives in the febrile stage of continued fever, even of a sthenic form, particularly if taken immediately before the Crisis—an effect that is frequently observed, when Diarrhœa supervenes spontaneously towards the termination of an attack of fever. Still more dangerous is the administration of a purgative immediately before or shortly after the commencement of the cold stage of ague; a mild case having sometimes been converted into a malignant one, the same as after the employment of an emetic. Even when administered at the right time—*i.e.* during the intermission—cathartics are found to be inefficacious, sometimes hurtful. Hence, in the intermittents of 1661-4, the attempt, says Sydenham, to cure the disease by purgatives was dangerous. Their employment would also require caution, when signs of inflammation of the intestinal mucous surfaces exist. They might, by aggravating this morbid state of the vessels, tend to lock up, instead of to remove, the poison there present. In other cases, as when the inflammation is of the adynamic form, their use may not only be allowed, but is generally beneficial.

The third class of remedies now under discussion are Diaphoretics, which have been employed, in all the different forms of fever, from time immemorial. Their use is restricted, however, to the febrile stage. There can be no difficulty in understanding their *modus operandi*. As Sydenham long since remarked: "The treatment by sudorifics may be said to be, properly speaking, Nature's own method, by which she expels the febrile matter out of the body." That this is the true explanation of their action, admits of no doubt, perspiration being an exudation rather than a secretion—the channel by which the watery part of the blood escapes out of the system, when in excess. Hence we understand, why morbid matters, when present in the blood, should escape by this channel rather than by any other, when left to the unaided powers of Nature. It is only in this way, that the beneficial effect of diaphoretics in fever can be understood; for perspiration, *per se*, when profuse, might be detrimental by weakening the patient, and rendering the blood thicker than before. Diemerbroeck and others, thinking that the perspiration weakened the patients, recommended it to be arrested in Plague. But Sydenham tells us, that the contrary was the result—the patient being relieved and strengthened by the perspiration. Not only can we understand why Diaphoretics are useful in fever; but, also, why they are, almost exclusively, had recourse to in the febrile stage. The ancient Physicians, indeed, did not administer this class of remedies until towards the termination of the febrile stage. The Aphorism of Hippocrates, "that concocted matters only, and *not* crude ones, are to be eliminated," would appear to apply, as Sydenham observes, to sweating, rather than to purging. By this we may understand, that Diaphoretics are not to be employed until towards the termination of the attack.

Although the most innocent, perhaps, of all the remedies employed in the treatment of this class of diseases, they ought not to be used indiscriminately. It has been concluded by many writers, that they sometimes, even in the sthenic form of fever—that form in which their use is more particularly indicated—favour the supervention of delirium, and increase it when present. So, also, in the low, asthenic forms of fever, in which the determination of the morbid matter to the surface is not so pronounced, they would not only be useless but might prove injurious. As the majority, and the most useful, of the class of Diaphoretics are nauseating remedies, and as all such remedies tend to weaken the patient, their employment, in these cases, would increase the tendency of the morbid matter to the interior rather than to the exterior. The employment of sudorifics must also depend on the type of the disease, as it would be useless to attempt the expulsion of the morbid matter by the skin, when Nature chooses another and a different channel for its exit. Hence, as Sydenham remarks, it is useless to administer sudorifics in the Plague, when there is vomiting and diarrhœa—occasioned by the morbific matter being thrown back on the internal organs. As soon, however, as the febrile matter reaches the circumference of the body, or re-action has taken place, they may then be given with advantage. In intermittent fever, also, diaphoretics are generally useless, sometimes injurious,—a result we might have expected, *à priori*, and from the simple fact that, in Ague, perspiration takes place to a considerable extent, at the end of each paroxysm, without producing any effect on the course of the disease. We need not be surprised, therefore, to find, that the Quartan agues treated by Sydenham, in this way, took six months for their cure. Not only are sudorifics useless in ague, but they may even be detrimental. Speaking of the diseases of 1675 to 1680, Sydenham states, he was convinced, as the result of many years' experience, that it was dangerous to attempt the cure of Tertian and Quotidian Agues by sudorifics alone. And he adds, if these remedies are pushed too far, they may convert an intermittent into a continued fever. This result would seem to confirm the conclusion before drawn,—viz., that the morbid matter in ague does not pass, under ordinary circumstances, into the secerning capillaries. When it does, the type of the fever becomes changed.

The last class of remedies that we have to consider are Tonics and Stimulants. The former cannot be considered as general remedies for fever, their employment being almost exclusively confined to the treatment of Ague. They have been occasionally administered in the mild forms of remittent fever, and in certain cases of the purely adynamic form of continued fever: but these are exceptions to the general rule. It is principally as Tonics, during the convalescence, that they have been resorted to in these diseases. This will not be surprising when we find that, even in ague, this class of remedies can only be successfully employed during the intermission. The reason is self-evident. It has been before concluded, that the febrile matter is situated in the venous and portal systems during the intermission of ague. There must also exist a state of greater or less congestion of these vessels, at this period, as shown by the fact, that intermittents, when not speedily arrested, invariably produce enlargement and disease of the Liver and Spleen. A Tonic, therefore, administered under such circumstances, would, by constringing the vessels, tend to remove this congestion; and thus enable them to expel their contents with more ease, by the natural outlet for extraneous, or morbid, substances in the venous system—viz., the bowels. The secretion, as it is usually termed, from the large intestines is rather an exudation, and would appear to come, or, at least, the principal part, directly from the abdominal veins. Hence it is, that a Tonic will frequently act as a purgative, particularly when first administered* These conclusions will be confirmed, if we consider which Agents of this class have been the most useful in ague. Although nearly every kind of tonic has been employed for the cure of ague—mineral and vegetable—the most successful have undoubtedly been the vegetable tonics. As these possess little or no astringent properties, they favor, more than any other tonic, the secretions and excretions of the intestinal canal. Mineral tonics, and, also, those vegetable ones, possessing astringent qualities, act, undoubtedly, in the same way, at first; but they soon produce the opposite effect.

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* That such is the action of these remedies, we may infer from the fact that, when tonics fail to produce any beneficial effect, the disease is frequently cut short by the employment of purgatives.

Hence the yellow bark, which is the least astringent, is more efficacious than the other kinds; and hence quinine, which possesses no astringent property, is more efficacious than any. Before the discovery of Quinine, it was usual to add stimulants to the Bark, by which its astringent properties were lessened or obviated, and its efficacy enhanced. That the Bark acted indirectly and not directly—by expelling a morbid matter out of the system, not by restoring the lost vital powers, as is generally supposed—would appear more than probable from the circumstance that, when administered at an improper period, or when too long continued, unfavorable symptoms generally arose.

When administered during the febrile stage, the Bark is almost invariably found to increase the intensity of the symptoms, if not to prolong the duration of this stage. Hence the cause of the almost universal administration of bark during the remission of ague. Its employment, even at this period, requires care, for when given immediately before the cold stage, serious results sometimes follow—a mild case being frequently converted into a malignant one by the supervention of vomiting, purging, and collapse. Sydenham tells us, that the Peruvian Bark fell into disuse soon after its first introduction. One of the causes was, that several fatal cases occurred after its administration—the supervention of the febrile stage having been prevented:

he was therefore induced to administer it immediately on the termination of the fit, by which these ill results were obviated. Precisely the same result—death—is sometimes observed by the administration of a purgative at the commencement of the cold stage: the poison being retained in the pulmonic and abdominal organs, instead of being carried forward into the arterial system—a less dangerous position, as has been pointed out in the previous chapter. So also when, from failure of the remedy to cure the disease, it was continued for too long a time, weight and oppression at the stomach, imperfect digestion, loss of appetite, and prostration of the vital powers, were generally produced. These effects may be ascribed, not so much to the debility produced by over-stimulation, as to the suppression of the secretions, by the subsequent action of the bark, and the consequent exhaustion of the nervous system by the retention of the poison in the blood. To the same cause we may, no doubt, ascribe the congestion and enlargement of the Liver and Spleen, so common in protracted cases of ague; effects which, otherwise, ought to be prevented by the administration of a tonic.

Although not the same always, injurious effects are also observed after the employment of quinine. Every one who has taken this drug, in heroic doses, must be aware, that certain symptoms are apt to arise—such as ringing in the ears, giddiness, confusion in the head, &c.—effects that must be ascribed to depression of the nervous system. In other instances, as when the remedy is given in still larger doses, or continued for too long a time, death takes place suddenly by, what may be termed, nervous apoplexy. In Italy, these results, or *accidentes*, as they are there termed, are very common; and are invariably ascribed to the employment of the drug in large doses, and to the sudden suppression of the fever.* It is also certain, that the conversion of simple and regular attacks of ague into irregular and malignant ones—more particularly that form which may be termed choleraic—must be ascribed, in many cases, to the peculiar properties of quinine. In the hospital of St. Louis, Paris, the administration of quinine some years since, was followed by the supervention of vomiting, purging, and all the other symptoms of a regular attack of cholera. This having occurred more than once, and one of the cases having terminated fatally; the attendant Physician was induced to withhold the use of the remedy, and to resort to others, for the cure of the disease, in those cases which subsequently presented themselves. A similar result was witnessed in the practice of my Friend, Dr.

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* Many cases have been recorded, in which death occurred after the administration of Quinine, in the ordinary doses. Vide Med. Gazette, No. 32, p. 430, American Journal of Med. Sciences, April, 1847, pp. 292 and 515, and Philadel. Med. Exam. April, 1847.

Sauch, physician to the General Hospital, Barcelona: ordinary attacks being converted into malignant ones. As these attacks were speedily relieved and cured by the employment of a different remedy, proof would thus appear to be afforded that they were caused by the Quinine. In a severe epidemic, which visited one Town, in the south of Spain, in which the same phenomenon was observed, the medical Practitioners, not considering that the supervention of the above symptoms—viz., the vomiting, purging, and collapse—was to be ascribed to the quinine, still continued to persevere in its use. But so regularly and so certainly did the same effects follow, after the administration of the medicine, that these Gentlemen were at last convinced of its being the cause; they therefore relinquished the quinine altogether, and trusted to general remedies for the cure of the disease, after which these results were not observed. Pereira states, that quinine sometimes causes griping pain and heat in the abdomen, with vomiting and purging, ptyalism, a febrile condition of the system, head-ache, giddiness, somnolency, delirium and stupor.* Like all the pure bitters, when taken in large quantities or for too long a time, this drug produces a sedative effect on the nervous system: and it is to this effect that we may refer the results just described.† If so, we can readily understand, why quinine, when taken in too large doses, should sometimes prove so injurious; the effect on the nervous system being precisely the same as that of the Agent productive of ague, and other forms of fever. It is thus evident that quinine, although more valuable than the bark, in the majority of cases, is the reverse in others; while the very properties that render it beneficial in the one case, make it inefficacious or dangerous in the other.

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* Mat. Med. vol. 2.

† Schlockow found, when Quinine was given to Frogs, that the

We may now turn to the class of stimulants, which have been employed for the cure of nearly all forms of fever, both alone and combined with other agents. Like tonics, they act as excitants on the nervous system; stimulate the heart to increased action, and promote the different secretions. Hence their beneficial effect in mild, simple, and uncomplicated, cases of fever; being precisely those in which their employment can be most readily dispensed with. Formerly, they were generally combined with the Bark in the treatment of ague, but, although only employed in the intermission—and their use is necessarily restricted to this period—they were apt to increase the intensity of the febrile stage: more especially when they failed to

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action of the heart became diminished, while the muscular tissue lost its irritability. The respiration, also, becomes irregular and intermitting, and the locomotive movement tottering and unsteady.*

* Schmidt's Jahrbuch. Vol. 113, p. 121.

aid in the elimination of the poison, or when this was not accomplished by other means. In continued and remittent fevers of the sthenic form, stimulants are contra-indicated, excepting in the last stage, or when the vital powers begin to fail. It is doubtful, however, if any effect is produced in such cases: if there be, it will be so transitory, and the depression will be so much increased subsequently, that the recovery of the patient can hardly be ascribed to their employment. The same argument will apply to the purely adynamic form of fever, as has been more particularly shown in the Introductory Chapter. But there is one class of stimulants, the anti-spasmodic, the action of which is different to that of ordinary stimulants, as is, also, their employment. If resorted to at that period when, as has been inferred, spasm is produced in the capillary vessels by the entrance of an extraneous matter, these agents may prove beneficial by relaxing the mouths of the secerning vessels, and thus allowing of the escape of the poison. Hence æther has been successfully employed in the cold stage of ague, and at the commencement of the febrile stage of continued fever. After this, when the contraction of the vessels becomes confirmed, the employment of anti-spasmodics is useless, if not detrimental.

There is another method, the reverse of the preceding, which is frequently adopted for the cure of continued fever—the anti-stimulant. Of the remedies belonging to this class, blood-letting and evacuants are considered to be the chief: but the action of these agents can be, and has been, in fact, explained on other and different principles. Tartar emetic is also regarded as an anti-stimulant, although it belongs, in reality, either to the class of evacuants, or to the class of diaphoretics: or to both. In small doses, the action of Tartar Emetic is confined to the skin; when given in large doses, it acts freely, not only on the skin, but, also, on the intestinal canal. Another anti-stimulant method consists in the abstinence from solid food, and in the employment of cooling drinks with mineral or vegetable acids. The last are doubtless beneficial in the majority of fevers, but their efficacy must be ascribed to specific, not anti-stimulating properties, as will be more particularly pointed out hereafter. As regards the mineral acids, they act as tonics and astringents, and are, in general, productive of harm, not good, by locking up the poison in the system. Hence it is, if cooling medicines be given too freely in continued fever, that the crisis, according to Sydenham, instead of occurring on the 14th day, as under other circumstances, is procrastinated until the 21st. The real anti-stimulant method of treatment is that of M. Broussais; and is comprised in the application of leeches to the abdomen, and the restriction of the patient to a rice-water diet. But this so-called method of treatment is merely leaving the case to Nature—the first Physician. That she is able to effect the expulsion of the febrile matter, in slight cases, particularly when aided by warmth, and warm drinks to promote perspiration, there can be little doubt. But she is unable to effect this object in severe and malignant cases, and becomes overpowered and prostrate unless aided by the efforts of Art.

If the preceding arguments have any weight, we must conclude, that the remedies we have now been considering act only, when productive of beneficial results, by expelling some morbid matter out of the system. It is the only way in which we can account for the action of such agents, many of which possess diametrically opposite qualities—for instance, antiphlogistics and stimulants or tonics. The employment of these opposite agents, it is to be remembered, does not always depend on a variation in the type of the disease; both classes being sometimes employed in the same disease, and at the same time; and yet the ultimate result is the same—the cure, or recovery, of the patient. We have had an example of this already, for while Rasori was adopting the anti-phlogistic, or anti-stimulant, mode of treatment, for the petechial fever of Genoa, the other Practitioners in the town were administering stimulants. The result would not, of course, be precisely the same, one mode of treatment being more successful than the others. Still, one method would not be attended with entire success; or the other with total failure, otherwise the latter would be speedily abandoned. That these various agents act indirectly, not directly, can be shown in another way. The same remedy is frequently found to vary, not only in the different forms of fever, and in different stages of the same fever; but, with different patients, in the same disease, and under precisely the same circumstances. On the other hand, a variation has been remarked, not with particular individuals, but in particular epidemics, or in certain seasons and years. Cœlius Aurelianus tells us that, "in the *pleurisies* at Rome and Athens, bleeding was prejudicial: but beneficial at Parium and on the Hellespont."* Lancisi also states that, "in the epidemic pleurisy, which prevailed at Rome in 1709, bleeding was found to be useful at one period, but injurious at another."† These anomalies have been particularly dwelt on by Sydenham, who remarks:—"The very treatment by which you cured patients in the middle of the year, would, perhaps, be destructive to them at the end of the year."‡ Thus, speaking of the fever in 1665 and 1666, he states that, "the diarrhœa, which could be prevented by the exhibition of an emetic, in the fevers of other years, appeared to be increased in the fever of these years, by the employment of the same agent." And he adds :—" Under, therefore, this so great obscurity, or ignorance, of these things, nothing appeared more important to me, when any new fevers made their appearance, than to wait a little, and only to proceed cautiously and slowly, especially in the use of strong remedies." The same result has been observed with the bark and quinine, both which have entirely failed in particular cases and in particular years. The cause of the variation, in these instances, is, of course, to a certain extent, hypothetical. We may presume, however, that it is to be referred to a difference, although unperceived, in the type of the disease—to the greater or less quantity of poison present in the system, or in particular parts of the system. Under such circumstances, remedies would necessarily vary in effect; the remedy that would be beneficial in one case or at one time, being inefficacious or detrimental at another.

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* De Morbis acutis et Chronicis. Lib. 2, cap. 22.

† Historia Romanæ Epidemiæ.

‡ On Epidemic Diseases, p. 29.

It is unnecessary to extend these remarks. It must be evident, from what has gone before, that there is only one way in which we can account for the *modus operandi* of the agents we have now been considering. This is, the introduction of a morbid substance into the blood: its retention at one time, and its expulsion at another. While, therefore, the theory affords a valid explanation of the operation of those agents, which have usually been employed in the treatment of diseases, from time immemorial;

the treatment, on the other hand,—its efficacy at one time, its inefficacy at another—confirms the theory previously enunciated. More than this, we find that the theory of disease, entertained by the Fathers of the Medical Art—absurd as that theory is in some respects—is nearer the truth than any that have since been proposed.* It is no less apparent, looking at the uncertain effect of the remedies hitherto employed, that a better, more certain, and more scientific, mode of treatment is urgently required. This feeling has been expressed by many writers, and more especially by the Father of English medicine — the English Hippocrates. "Although," says Sydenham, "that seems to be the best method of curing acute diseases, which, after Nature herself, promotes some certain kind of evacuation—which method, by promoting the said evacuation, assists her, and thus necessarily contributes to the cure of the disease—it is, nevertheless, to be wished, that the patient might have the advantage of a shorter road to health, by means of specifics, if any such can be found; and what is of more importance, that he might be placed beyond the reach of those evils, which follow the errors that Nature often unwillingly makes in expelling the cause of the disease—even when she is aided in it, effectually and skilfully, by the attendant Physician." No one can dispute the force of this reasoning; the only question is, has any specific been found for this class of diseases. As they are produced by the introduction of a poison into the system, there can be no specific for them, unless it be, at the same time, an antidote: that is to say, unless it is capable of uniting with, neutralizing, and rendering inert, the deleterious matter productive of all the morbid effects. Whether such an agent has been discovered, will be considered in the next chapter.

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* "The most praiseworthy enterprise," says Boerhaave, "would be to reconcile the pathology of the Ancients with the Physiology of modern Authors." *Method. Studii. Medic.* pars. 1, Pathologia, p. 573.

CRAPTER III.

THE ANTIDOTAL TREATMENT OF FEVER.

THE finding of an antidote, or specific, for all the ills to which flesh is heir, has been, like the discovery of the Philosophers' stone, the dream and the ambition of Physicians, Philosophers, and others, from the foundation of medicine to the present day. Mithridates, the celebrated King of Pontus, wrote a work on the Alexipharmics,* which Pompey, after the conquest of the Kingdom of the above Prince, caused to be translated into Latin. Certain of the Sophists, who studied, if they did not practise medicine, attributed diseases to a general morbific cause, or principle, which it was necessary, they thought, to destroy. Hence the Alexipharmics, the Mithridates, the Theriacs,† and other compositions, containing a great number of ingredients, some one of which, it was imagined, might be able to accomplish the end in view. But all these reputed specifics have now been repudiated and forgotten. As chemical knowledge increased, and the composition of substances, organic and inorganic, became known, Chemists were enabled to ascertain, by direct experiment, what substances were capable of combining with them, or, at least, a certain number of them; altering their properties, and, in the case of poisons, destroying their virulence. To the last-named agents the term *Antidote* is now restricted.[*] Although still unacquainted with any antidote for a great many poisons—and these some of the most virulent—this branch of the healing art has been dignified with the title of "The Science of Toxicology:" the practice of which is confined to those cases, in which deleterious substances have been taken into the stomach either by accident or by design. Under the idea that diseases are produced by the operation of a poison in the system, certain substances have been

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* Αλεξω to help and φαρμακον medicine, the name being applied to substances which were supposed to exert a specific action on poisons; or, which had the power of neutralizing and destroying their dangerous properties.

† Theriaca (theriac) was a mixture of about 70 ingredients, some of them quite inert, and others antagonistic to one another. It

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was prepared in the form of an electuary, and the word *treacle*, a corruption of *theriacal*, and which, at first, meant an electuary, is derived from it. A preparation of this name continued in repute until recent years; and it is not long since, that the Pharmaciens in Venice, France, and Holland, were compelled to prepare it in the presence of the Magistrates or other Authorities.

[*] Αντι and δοτον, that which is given against (poisons): either as a remedy or preventive.

employed, in recent times, as antidotes, although no evidence had been advanced, respecting the nature of the morbid matter; it was impossible, therefore, to ascertain which agents were best fitted to unite with it, alter its properties, and destroy its virulence. Those Theorists, who inferred that fever was due to putridity of the blood, were induced to administer Musk, the Mineral Acids, Bark, and other supposed Antiseptic substances. But such remedies do not act as antiseptics, or Antidotes, in the living body: they all belong to the class either of Stimulants or of Tonics, to which their action is alone to be referred. Since the discovery of Quinine, and its successful administration in ague, many writers have been led to infer, that it possesses specific properties in this disease. This inference is not a valid one. The action of quinine, like the Bark from which it is derived, acts on general, not on specific principles. Although deprived of the stimulating and astringent properties of the bark, it acts, nevertheless, as a Tonic in moderate doses; and is employed, as such, daily, in a variety of complaints. It is easy, therefore, to explain its *modus operandi* without any reference to specific properties. This, in fact, has been already done in the previous chapter. As Orfila has remarked, " There is a wide difference between those medicines, that *specifically* answer to this or that indication of cure, and which, if we employ them properly, expel the morbid influence;

and those which specifically and immediately cure a disease, without any regard to this or that intention, or curative indication. For example, mercury and sarsaparilla are usually considered to be specifics in *lues venerea*, but which ought not to be considered proper and immediate specifics, unless it could be shown, by valid and irrefragable arguments, that mercury had accomplished this great work, without exciting salivation, and sarsaparilla without producing perspiration." * That quinine possesses no specific properties can be shown by other circumstances.

In the first place, quinine, when it fails to arrest the progress of the disease within a certain time, is powerless afterwards. In the next place, in particular years or in particular visitations, independently of isolated cases, it is entirely inefficacious. After the subsidence of the epidemic cholera in Spain, in 1835, Agues became epidemic; not only prevailing in localities where they are endemic, but in others also, as in the towns, while they assumed a more severe form than usual. To the surprise of the resident Practitioners, Quinine was found to have little or no influence in arresting the progress of the disease. Concluding, from this unusual circumstance, that the drug had been adulterated, a fresh supply was obtained from Marseilles, and subsequently from Paris: but the result was the same. It became necessary, therefore, to resort to other remedies, and to the old method of treating the disease, viz., by evacuants. Then, again, although arresting the paroxysm, this remedy is unable to prevent the supervention of a variety of effects, usually considered to be the *sequelæ* of ague. On the contrary, by arresting the paroxysm without curing the disease, these effects would appear to be favoured and promoted by the employment of the remedy; particularly when administered in too large doses, and for too long a time. And, lastly, it is, as previously shown, productive of other and fatal results, not observed when other remedies, and other modes of treatment, are adopted. Results like these are altogether incompatible with the idea of quinine possessing specific properties. In addition to these reasons, quinine is nearly or altogether useless in other forms of fever, and in other diseases produced by the same poison. This would not be the case, if quinine were a specific, for although it might not produce the same effect in every form of fever, and in every disease, it would still manifest its specific properties in each. As such, we must reject the claims that have been set up in favour of this now almost universal remedy.

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* Toxicologie, p. 19.

Up to the present time, no other Agent has been proposed or employed for this special object, with the exception of the one made known by me some years since: the discovery of a specific for fever is therefore, or was, until lately, a *desideratum*. Such a discovery has, in fact, been regarded as hopeless and imaginary. " The study of morbid principles," remarks M. Bernard, " can, therefore, alone enable us to discover the means of curing the disorders to which they give rise; and for this purpose, two different systems might be adopted: firstly, to neutralize the morbid agents: and, secondly, to eliminate them from the body. In the present state of our knowledge, we possess no means whatever of neutralizing their actions: as in the case of poisons, they must be expelled before they cease to act. To this result, do all the efforts of Nature tend; and to this result also, must all the Physician's endeavours be devoted."* It does not follow, however, because we have hitherto been without such an Agent, that the search for an Antidote would be useless. On the contrary, we may be certain that such an Agent does exist, for all poisons are capable of being destroyed and rendered innocuous by combination with other substances.† Sydenham, while expressing his regret that no specifics, in the proper sense of the term, were then known, adds: "Nevertheless, I doubt not, that out of the exuberant fulness with which Nature abounds and flows over—so it pleases the wise and great Artificer of all things—for the preservation of the human race, provision is likewise made for the cure of the chief diseases that afflict them: and that by such medicines as are within the reach of all, and in every clime." (Opera p. 19.) The difficulty in the present instance is, that the febrile matter, unlike the ordinary poisons, has never been collected or analysed. Being, therefore, unacquainted with its chemical properties, or composition, we are unable to ascertain, by experiments conducted without the body, what Agents are capable of combining with it, altering its properties, and rendering it inert. All we can do, under these circumstances, is to trust to chance, to analogy, or to induction, in our search for an antidote; to each and all of which we have been indebted for many discoveries in medicine, as well as in the Arts and Sciences.

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* Medical Times and Gazette, July 14, 1860.

† Had Professor Bernard been acquainted with my previous investigations, he would, perhaps, have drawn a different conclusion. And yet my work on the epidemic cholera, in which a true Antidote for this disease was made known, had been translated into French 24 years before. Thus slow is the progress of medical discoveries.

Having, many years since, had my attention directed to the subject of Malaria—its production and nature—and having been a great sufferer from its operation, I remarked that, if not produced by the same cause, some of the effects were similar to those observed in the class of poisons termed septic. I was therefore led to infer, that Carbon and its compounds would be, or ought to be, antidotes for the poison productive of these effects, and specifics for the diseases arising from the operation of malaria in the human frame. As is familiarly known, if a piece of putrid meat be suspended in an atmosphere of carbonic acid gas, it is soon rendered sweet. Mr. Henry, the celebrated Chemist, and some other Philosophers, consider that the gas serves as a menstruum for the putrid matters, which it attracts from the meat: they having remarked, that the gas becomes offensive, although the meat was sweetened. This result may, perhaps, be ascribed to the over-saturation of the gas, not having been used in sufficient quantity: as we can hardly infer, that it acts as a mere menstruum for the putrid matters. It would be more in accordance with all the facts presented to our notice to conclude, that the putrid matter is not only attracted by the gas, but that it is, also, decomposed by it, and rendered innocuous. This is undoubtedly the case with the common form of carbon or charcoal, which has the property not only of absorbing, but of decomposing, many gaseous compounds. Not having had an opportunity previously, I took advantage of a visit to Spain, in 1834, to institute a series of clinical experiments by administering my presumed antidote in a certain number of cases of Intermittent fever—the prevalent disease in that country.* This disease was selected for several reasons. In the first place, it is the most simple form of fever; and, in the next, it affords an opportunity for the administration of the remedy under three different circumstances; viz. in the cold stage, in the febrile stage, and in the intermission; not once only but repeatedly, and with the same individual. The importance of these circumstances will be evident as we proceed. I also had another object in view. Inferring, that the physiology of the cold stage of ague and of the collapsed stage of cholera is identical, at the same time that the collapse of the former is incomplete, I considered it to be the best fitted for elucidating the phenomena observed in the two diseases. The general results then arrived at were given in the 1st Edition of my work on the Epidemic cholera, in the chapter on the physiology of this disease.

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* It has been said, that scientific observation consists in remarking the effects produced by the unassisted efforts of Nature ; while

As carbonic acid was the agent employed, I inferred that, when taken into the stomach, it would be absorbed by the veins of that organ and of the small intestines, and be carried forward, with the circulating current, to the great venous centres and pulmonary organs, passing out of the system again with the expired air. This, at least, is the case with other gaseous substances,* and with the carbonic

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experiments, according to the well-known definition of Laplace, consist in disturbing the natural evolution of phenomena by direct interference . . "The Interpreter of the Works of Nature," says Leonardo da Vinci, "is experiment: that is never wrong."

* Vide Chapter 1, Physiology of the Disease, p. 95.

acid naturally existing in the system, it being now concluded, as previously mentioned, that this gas is not formed in the lungs, but in the systemic capillaries; whence it is conveyed, with the venous blood, to the pulmonic organs—that portion, at least, which does not pass out of the system by the skin. For reasons that will be apparent, by a reference to the Physiology of this disease, the gas was first administered about two hours before the usual commencement of the attack, so as to enable the patient to take 4 or 5 doses previously to the supervention of the cold stage. In these cases, the paroxysm was invariably shortened, while, if the remedy continued to be taken in the same manner, on the subsequent days of the accession, the disease was arrested in a shorter time, and with more certainty, than by any other known remedy. This will be evident by a reference to the cases hereafter given.

The effect of the remedy was next tried during the cold stage. If taken at the very commencement of this stage, some immediate benefit—such as shortening the paroxysm—was generally observed; although only one or two doses had been taken; as I considered it useless to continue the administration of the remedy, after the cold stage had fairly set in. In fact, in those cases in which the paroxysm commenced before the ordinary time, and shortly after the administration of the first draught, little or no benefit appeared to be experienced, although the regular number of doses were taken. This result is easily accounted for. When congestion of the venous system exists to a considerable extent, the absorption of liquids, and even gaseous substances, is prevented altogether, as the experiments of various Physiologists have demonstrated. The circulation of the blood, in the height of the cold stage, being altogether or partially suspended, a remedy taken into the stomach, at this period, would probably remain there, or, if absorbed at all, be retained in the abdominal veins. It could not, therefore, neutralize the matter, present, as has been inferred, during the cold stage of ague, in the great venous trunks, and in the pulmonic organs. More than this, it has also been concluded, that the greater part of the morbid matter is contained, at this period, in the capillaries of the lungs, which are in a state of constriction; here, then, would be another obstacle to the neutralization of the poison, even supposing that the remedy were absorbed and carried on into the venous centres. Nor could the carbonic acid gas be brought into contact with the morbid agent subsequently, as this matter, according to the deductions previously drawn, would be propelled into the arterial system; while the greater part of the remedy would escape with the expired air. Hence the failure of the gas to produce any apparent or immediate beneficial result, when administered during the cold stage of ague.

The remedy was next tried during the febrile stage, but the benefit derived was slight, scarcely perceptible at the moment. If given at the same period, on the subsequent accessions, these were gradually lessened in intensity and duration: but it took some time to produce permanent relief, or to effect a cure. The time, in fact, was so long, in the majority of instances, that I was obliged to have recourse to another and more speedy method of cure, and to administer the gas shortly before the accession. I could not, indeed, conscientiously keep a patient under treatment for weeks, when others, labouring under the same complaint, and of the same intensity, were cured in the same number of days.

A third series of experiments was then adopted, by administering the remedy during the intervals of the attack. When the gas was taken immediately after the subsidence of the febrile or sweating stage, the benefit was little more than what was obtained by the employment of the remedy at the above period. This was more particularly the case, when the intervals between the accessions were long, as in the Tertian and Quartan fevers. In proportion, however, as the period, selected for the administration of the remedy, approached that of the return of the paroxysm, so was the benefit experienced greater and greater—provided only that there was sufficient time for the administration of 3 or 4 doses of the gas, before the commencement of the cold stage. It was thus shown, by direct experiment, that the best time for the administration of the gas is precisely that which was selected by me, from deduction alone, in the first case of ague that I treated with this remedy. While, therefore, these results prove the truth of the theory, before enunciated; so, on the other hand, it is only by a reference to this theory, that we can understand the *modus operandi* of the remedy—its efficacy in particular cases, its inefficacy, or failure, in others. The experiments confirm the deductions, and these, again, offer a satisfactory and complete explanation of the results obtained by the administration of carbonic acid gas.

Having premised thus much, we may now proceed to a more particular detail of those cases, that were treated, not to illustrate the physiology of ague, but to show how much can be effected, for the treatment of this disease, by the employment of carbonic acid gas. It must be unnecessary to add the other histories; the general result, already given, is sufficient for the purpose.

CASE 1. Rita Garcia, when I visited her, had been in the hospital of Alicante for a month, suffering from a quotidian ague: but the disease had existed *between* 9 *and* 10 *months*, having proved refractory to all the remedies employed—quinine, bark, arsenic, steel, etc. The paroxysms were less violent than before; but there was considerable debility with great emaciation: and the patient complained of pain at the pit of the stomach, oppression at the chest, and want of appetite.*

The quinine, which the patient had been taking, was suspended; and she was ordered to take 30 grs. of Bi-carbonate of Soda, with 20 of Tartaric acid, in a state of effervescence, two hours before the usual time of the accession, repeating the dose every quarter of an hour, until 4 or 5 had been taken. As the accession commenced soon after taking the 3rd dose, no more was administered. The paroxysm was less severe, and of shorter duration, than before. On the following day, the patient took 4 doses of the medicine in the same manner, and at the same time. Coldness of the extremities, followed by slight and temporary heat of the surface, was alone experienced.

The same plan was pursued on the 3rd and 4th days, without the patient experiencing any unpleasant symptom, or the least return of the disease. The draughts, therefore, were discontinued. With the cessation of the attacks, the pain and oppression at the chest also disappeared: the appetite returned, and the patient rapidly regained her health and strength.

Case 2. Joaquin Gosalvez. This patient had been suffering for a month from Quotidian Ague, but he had only been in the hospital a few days, and had not as yet taken any medicine.

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* For date of attack and other particulars, see Table further on.

The patient was ordered to take the effervescing draughts in the same manner, and at the same time, as in the previous case; but as the attack commenced soon after the administration of the 2nd dose, the remaining draughts were not given. The duration and intensity of the Paroxysm were the same as on the previous occasions. On the following day, 4 draughts were administered, at the same period, at intervals of half an hour; and the patient escaped *without experiencing any attack,* or the slightest indication of its approach. The remedy was repeated on the three following days, when, as the patient had no return of the disease, it was discontinued.

Case 3. Antonia Alvoreha, during her convalescence from Cholera, in the same hospital, six months previously, was attacked with a Tertian Ague, which, with the exception of 15 days, had continued up to the time of her re-admission, in spite of the employment of quinine and other remedies. The attacks, however, were less severe than at first.

During the intermission, on the following day, two draughts were given, one in the morning and the other in the evening; and, on the next day, corresponding to that of the periodical return of the disease, four draughts were taken, in the usual way, but *no attack was experienced.* The same plan was pursued on the two subsequent days, when the medicine was discontinued; the patient having escaped the attack then, the same as before.

CASE 4. La Señora ——, who, on the breaking out of the cholera, seven months previously, had fled to a small island near the coast, was attacked, shortly afterwards, with a Tertian Ague. The disease continued, without any intermission, during the whole of this period, in spite of the administration of Quinine and Bark, for the greater part of the time; but the paroxysm was less severe than before, and the time of its return not quite regular.

The patient was ordered to take the carbonic acid in the way already pointed out; but, as the attack commenced immediately after the administration of the first dose, it was not repeated. The same result was experienced on the next occasion, in consequence of the cold stage commencing two hours before the usual time. As might have been expected, the duration of the attack was the same as previously. On the third occasion, being the day of the regular return of the fever, four draughts were taken at intervals of half an hour; and the patient passed the day without experiencing any attack. The same plan was adopted on the two following fever days; but, as the attacks did not return, no more draughts were ordered.

With the exception of the last, the preceding cases were kindly assigned me by one of the Physicians to the hospital at Alicante, whither I had proceeded, on the subsidence of the epidemic cholera in Barcelona, having heard that ague was prevailing epidemically in the district. It would have been a source of much satisfaction to me, to have had more cases to treat; but, as it did not appear to be agreeable to this gentleman to see patients, whom he had been treating six and seven months, cured, and radically cured, in the same number of days, I could not press this request on him. There were plenty of cases in the town, and being applied to daily for my advice by numbers of these, I was induced to undertake the treatment of several.* But there were difficulties out of the hospital as well as in, although of a different kind. These obstacles were the opposition and prejudice of some of, or all, the doctors in the town; and the propagation of the following story. A child having been missed, it was asserted, that it had been kidnapped, and taken to the English Consul's country-house, in order to furnish, by venesection, a daily dose *of blood*, which *I* had ordered for his son, supposed to be in a decline. Being in a town, in which the knife is used as freely as the fist in England, two sailors, belonging to Captain, now Admiral, Codrington's ship, having been assassinated in open day, and in a public square, on the day of my arrival; I thought it more prudent to look after my own safety than the cases of intermittent fever. I left Alicante, therefore, and proceeded on to Madrid. Curiously enough, a Spanish officer, with whom I travelled from Alicante to Valencia, and who imagined I was a merchant—and I was not at all anxious to undeceive him—related the story to me as an undoubted truth; and ridiculed the scepticism which I ventured, with all humility, to express on the subject. He, also, was suffering from Ague, and had an attack on the road; but I did not proffer my advice or my services on the occasion, *Merchants* do not usually understand the treatment of diseases!

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* Not having been able to watch these cases, or superintend the administration of the remedy, I do not attempt to give their histories. It is only necessary to add, that they were cured like the rest.

Although situated on a dry and arid plain, 1200 feet above the level of the sea, intermittents prevail at Madrid, and in the neighbourhood, the same as on the low and marshy plains of Valencia; and in the same intensity, although not to an equal extent. Being anxious to continue my investigations, and having some claim on the Spanish Government, on account of the services rendered by me during the prevalence of the epidemic Cholera; I applied for some cases of intermittent fever to be assigned me, in the general hospital of Madrid. This request was most graciously acceded to, and a Ward set apart for me, by the express command of Her Majesty, the Queen Regent. The following cases, selected from others, will show that the result was precisely the same as that which had been previously obtained. My object at this time, however, was to ascertain, if the theory I had formed, respecting the physiology of Ague, was a correct one; rather than to obtain fresh evidence respecting the efficacy of the remedy, as, on this point, there seemed to be little doubt. These histories, for the reasons already given, will not be added; but those only in which the remedy was administered at the proper time, and in the proper quantity.

CASE 5. Juan Seldran was admitted on the 22nd August, 1835, having been attacked, a week previously, with Tertian Ague. He was placed on low diet, according to the general rule of the hospital. On the 25th, being that of the accustomed accession, the patient took 4 draughts, at intervals of half an hour; commencing four hours before the regular time of the attack. This returned at the usual period, the cold stage lasting nearly the same time as before, but the fever only lasted half an hour, instead of three hours.

The remedy was given to the same extent on the next fever day, only commencing its administration two hours, instead of four, before the usual time of the accession. No other symptoms were observed than a slight increase in the temperature of the external surface, which lasted only about half an hour. The same plan was adopted on the next fever day, but no return of the disease having been experienced, the remedy was administered twice a day only, for a few days, and was then discontinued.

Case 6. Francisco Yanez had been admitted, a few days previously, with a Tertian Ague, from which he had been suffering for 17 days. Having been sent into my Ward, on the 25th August, 1835, he was ordered to take the draughts in the ordinary way, on the next fever day; but, in consequence of the attack having commenced some hours earlier than usual, the medicine was not administered. The cold stage lasted three hours and a half, and the hot stage, from six to seven.

August 29th. The cold stage commenced yesterday shortly after the second dose of the gas had been administered: as such, no more was taken. The cold stage lasted an hour and a half, and the fever 10 hours, but the intensity of the attack was less than before. On the next day of the accession, the patient commenced taking the draughts three hours before the expected time of the attack; repeating the dose every hour, until the cold stage had set in. Four draughts were administered. The cold stage lasted half an hour only, and the fever two hours—the symptoms, also, being much less severe than on the last occasion.

Sept. 1st. The medicine was repeated in the same manner as before; the only symptoms experienced being slight and temporary chilliness, followed by as slight an increase of heat on the external surface. The remedy was administered on the two subsequent fever days, in the same manner and to the same extent; but no return of the disease having been experienced, it was ordered to be discontinued.

Case 7. Pedro Trebiño was admitted on the 16th Sept., 1835, having been attacked 14 days previously with intermittent fever; at first, every other day, and, latterly, every day. He was ordered to take a draught on the following day, two hours before the usual time of the attack; and to repeat it every half hour, until 4 or 5 doses had been taken, or until the cold fit had set in.

Sept. 18th. The patient passed the day *without experiencing the slightest attack*, having taken five doses of the medicine. The same plan was pursued on the two following days, after which, as no symptoms of fever had been observed, a draught was ordered to be taken twice a day, for a few days. He was discharged on the 26th.

Case 8. Pedro Gonzalez was attacked, about a month previously, with quotidian ague. He had taken an electuary of bitters, which had the effect of arresting the paroxysm for a few days; but the disease having returned, he entered the hospital, on the 18th Sept., 1835. On the last occasion, the cold stage lasted half an hour, and the fever two hours.

Sept. 19th. The patient took the carbonic acid to-day in the accustomed manner, and passed the usual period *without any attack.* The medicine was continued for a few days, when the patient, at his own request, was discharged.

Dr. Roca, one of the Physicians to the Military Hospital, Madrid, who was pleased to adopt, at my suggestion, the plan of treatment now under consideration, having favored me with the notes he took on that occasion, I am enabled to add the following cases:—

Case 9. Manuel Jolan, a private in the 2nd Regiment of Guards, was admitted on the 18th August, 1835, with a Tertian Ague. He was placed under observation, no medicine being administered. On the 19th, the fever returned, the paroxysm lasting 4½ hours.

August 21st. Four doses of Carbonic acid gas were administered, shortly before the expected time of the attack, and "*even on this day,*" remarks Dr. Roca, "*he escaped the fever.*" On the 23rd and 25th, he took the same number of draughts, and on the 27th two only, without experiencing any attack. On the 28th, a purgative was administered; and, on the 9th September, he was dismissed—cured.

Case 10. September 2nd, 1855. "Blas Adan, a private in the Queen's own Regiment, was attacked, 11 *months previously,* with a Tertian Ague, in consequence of having plunged into the water while in a state of perspiration; and which has returned 5 or 6 times, *proving almost refractory* to the action of the Bark, quinine, and other remedies, in the hospitals of Logroño, Burgos and Palencia. Lastly, in the journey which he made from Vittoria to Madrid, it has annoyed him throughout the whole of the road, with more severity than on any other occasion. Having, on his arrival in this Capital, the 2nd September, presented himself at the hospital, he appeared unusually pale, with prostration of strength, want of appetite, furred tongue, thirst and somnolency. On the following day, the 3rd, the accession commenced at 11 a.m.—the cold stage lasting an hour and a quarter, and being very intense in degree, with the skin uncommonly rough and dry; pulse hardly perceptible, but quick; heat in the abdomen, particularly in the epigastric region; unquenchable thirst; tongue very dry, and great pain in the head with inclination to sleep."

"On the 4th, he took four doses of the Carbonic acid, commencing with the first at 9 a.m. (two hours before the regular period of the accession), and repeating the draughts every half-hour. At the last dose of the medicine, the patient experienced a particular sensation along the spinal column, which he explained by saying, it appeared as if some one were pouring water on his shoulders, and blowing on his head. After lasting 8 or 10 minutes, the paroxysm gradually disappeared *without more cold-*

ness, and without being followed by fever, or other perceptible phenomena." The patient continued taking the remedy on the 7th, 9th and 11th in the same manner; and having had no accession, or fever, he was, on the 28th, dismissed from the hospital,—cured.

Case 11. Sept. 6th, 1835. "Manuel Martin Estremeño, a private, was admitted into the hospital on the 26th July, with a quotidian Ague. This was arrested, in its course, by the administration of quinine, but *the fever returned at the end of* 15 *days*."

"On his re-admission, the gas was administered to him, four doses each day, commencing *half an hour* before the usual time of the accession. The paroxysms gradually diminished both in intensity and duration; and disappeared altogether, on the 6th day, without any relapse having been observed subsequently."*

Case 12. Sept. 15th, 1835. "Pedro Gutierrez, a recruit, entered the hospital on the 26th August last, with a Quartan Ague. He had an emetic, was bled, and then took the Bark, and its preparations, by the help of which the fever was cut short. *Having returned*, after an interval of 8 days, the carbonic acid was administered to him, in the before mentioned manner, and, at the second attack, the fever entirely disappeared."*

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* The remedy, in this case, was improperly administered: the first dose should have been given *two hours* before the expected time of the attack. To this circumstance, we may doubtless attribute the longer duration of the treatment.

Dr. Sauch, Physician to the General hospital in Barcelona, was also induced, at my suggestion, to employ the same remedy in a certain number of cases, and, as he informed me, with a similar result. In one case, in which the sulphate of quinine had been previously administered, the disease was suddenly converted from an ordinary into a malignant form, by the supervention of vomiting, purging, and partial collapse. The carbonic acid being had recourse to immediately, these dangerous symptoms were speedily and completely removed, and, in addition, the fever also, as no attack was experienced subsequently.

The result of the treatment, in these cases, was considered, by many Practitioners, to be so singular and remarkable, that they concluded there was some peculiarity in the fever that year. As it appeared epidemically, shortly after the subsidence of the epidemic cholera; and as it assumed a form so unusually severe and rebellious; these Inquirers imagined, that the fever was due, not to the ordinary cause, but to the same cause as that which produced the epidemic cholera. This inference appeared to be confirmed by the fact, that the same remedial Agent, which had been so successfully employed in the treatment of cholera, was found to be equally beneficial in these cases of fever. I was therefore anxious to embrace the first opportunity, that presented itself, of treating other cases of intermittent fever, in a locality, and at a time, when the ordinary cause of this fever could alone be in operation. Being in Rome, I applied to the then Secretary of State, Cardinal Antonelli, to be allowed to treat some cases of intermittent fever in one of the hospitals of that city. This letter being referred to the Administration of Civil hospitals, some cases were assigned me in the hospital of Santo Spirito: and the following histories will give the result then obtained.

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* The preceding histories were given in a Pamphlet, published in 1836, under the Title : "The efficacy of Carbonic acid gas, in the Diseases of Tropical Climates."

Case 13. Filipo Gentile, Æt. 21. April 6th, 1858. Was attacked, on the 4th inst., with a Tertian ague; the duration of the cold stage being half an hour, that of the hot two hours. The subsequent transpiration was slight. Having entered the hospital yesterday, and being placed under my care, he commenced the treatment to-day.

Three hours before the time of the expected attack, he took an effervescing draught, repeating the dose every half hour until 4 draughts had been taken. *No attack was experienced.* The remedy was administered in the same way on the 8th; and as the fever did not return, he left the hospital on the 10th.

Case 14. Pasquale Borgini, Æt. 21. Having been attacked, on the 5th of April, with a Tertian Ague, he entered the hospital on the following day. The duration of the cold stage was 15 minutes only, and that of the febrile stage one hour, followed by slight perspiration. He was treated, on the next fever day, in the same way as the former patient, *had no attack*, and left the hospital on the 10th.

CASE 15. Luigi Frati, Æt. 20. April 8th, 1858. This patient was attacked, on the 5th inst., with a Quotidian Ague, the paroxysm lasting 5 hours. He entered the hospital yesterday, and commenced taking the gas as soon as the sweating stage had entirely passed off, repeating the dose every four hours until the return of the paroxysm.

April 10th. No cold stage was observed yesterday only the febrile stage; while this was of shorter duration, and less intense, than before. The same treatment was pursued to-day, but no attack was experienced. Having had no return of fever, the patient left the hospital on the 14th, having remained some days longer than he would have done on account of a slight eruption, which made its appearance during the treatment.

CASE 16. Domino Bicarelli, Æt. 27. April 8th, 1858. This patient was attacked, two months before, with a Tertian Ague, which was arrested by the administration of Quinine: but having returned, after an interval of a few days, he entered the hospital on the 6th inst. The attack commenced to-day shortly after midnight, the cold stage lasting two hours, and the hot stage, *twelve*, attended with great thirst, a hard and strong pulse, and severe pain in the head. The perspiration was abundant, and continued longer than usual. He was ordered to take the gas, commencing two hours before the usual time of the accession, and repeating the draught every half hour, until four doses had been taken.

April 10th. *The patient escaped without an attack* to-day; not the slightest symptom, either of coldness or of fever, having been experienced. To continue the medicine as before.

April 12th. Not having had any return of the fever, and being anxious to leave the hospital, the patient was discharged.

Case 17. Luigi Bartoli, Æt. 13. Was admitted April 8th, 1858, having been recently attacked with Tertian ague. The duration of the cold stage was two hours: that of the hot one only. The remedy was administered in the same way, and to the same extent, as in the preceding cases; but the patient experienced no attack after his admission, and he left the hospital on the 12th.

Case 18. Santolo Sartzoli, Æt. 23. April 8th, 1858. This was a double, or duplicated, Tertian, the disease having commenced 3 days previously. The duration of each paroxysm was 4½ hours, with an interval of three hours. The gas was administered during this period, to-day; but the attack returned at the usual hour; the intensity and duration of it however, were less than before.

April 11th. Yesterday, the remedy was taken in the same way as before, viz. in the interval between the two paroxysms. The patient passed the day without experiencing the slightest attack, or unpleasant symptom. The medicine was repeated on the following day, and having had no return of the disease, he left the hospital on the 13th.

CASE 19. Luigi Marinelli, Æt. 22. 8th April, 1858. This patient, who entered the hospital yesterday, had been attacked 10 days before with Quotidian Ague—the cold stage lasting one hour, and the hot, from two to three hours, followed by an abundant perspiration. A purgative was administered yesterday, in accordance with the regular practice of the hospital, and he was then placed under my care. The usual attack, which lasted nearly four hours, occurred to-day.

April 10th. Yesterday, the patient took four doses of the gas previously to the accustomed period of the accession, which did not return; slight symptoms of fever, which lasted about an hour, being alone experienced. Having had no return of the disease he left the hospital on the 15th.

CASE 20. Luigi Vincino, Æt. 24. April 14th. This patient had been attacked with fever two months before, but the disease was arrested, after three accessions, by the administration of Quinine.

Ten days since, the disease returned, presenting, at first, a Tertian form, and afterwards a Quotidian. The duration of the last attack was 6 hours.

April 15th. He took the medicine yesterday, in the usual way, and in the usual quantity, but the paroxysm returned, although it lasted a less time than before, while the symptoms were less intense. The remedy was repeated on the two following days, but no attack was experienced; and the patient left the hospital on the 19th.

CASE 21. Natali Cochiotti, Æt. 11. The fever, in this case, commenced three weeks previously: at first, every day, and then every other day. The attack, however, was slight, and it yielded to the first administration of the remedy.

CASE 22. Antonio Piacentini, Æt. 13. This lad, who was admitted on the 15th April, was suffering, like the former, from a slight attack of Tertian Ague, which had existed for six weeks previously to his admission. He took the gas in the ordinary way on the 16th, and experienced slight symptoms of fever, at the usual period of the accession, but no coldness or other symptoms. He escaped the attack altogether on the next fever days, and he left the hospital, on the 19th, cured.

CASE 23. Pietro Capparucchi, Æt. 22. 16th April, 1858. This man, who entered the hospital three days since, had suffered from an attack of Tertian Ague two years before—the disease continuing, without intermission, for a whole year—from Sept. 1855 to Sept. 1856. Last year, he was attacked again, and at the same period, in the month of Sept.; and the fever has continued, without intermission, to the present time. In the previous attack, he took Quinine regularly, as, also, other remedies; but being in a country district, beyond the reach of medical aid, he has taken no medicine since the commencement of the present attack. The paroxysm is severe, the cold stage lasting two hours, and the hot, eight hours, without being followed by the usual perspiration, or the sweating stage. At first, the fever was Tertian, latterly, it has assumed a Quartan type, but with this peculiarity. Each day, during the intermission, the patient has a slight attack of fever, without being preceded by the cold stage: thus assuming the form of an imperfect *triple* quartan.

April 17th, 1858. The patient commenced taking the gas yesterday—being the regular day of the paroxysm—four doses having been administered to him previously to the expected period of the attack. *Not the slightest sign of coldness or of fever was observed.* No febrile symptoms were experienced on the two following days—those of the intermission—and no attack of Ague on the third, or on any subsequent day. He was discharged on the 21st, being anxious to return home.

CASE 24. Filipo Cuchetti, Æt. 33. 19th

April, 1858. This individual was attacked last year, in the month of August, with a Tertian Ague, which was arrested, after 8 days, by the administration of quinine. But the fever returned again in 6 weeks, and continued for a month, in spite of the employment of quinine, purgatives, &c. Last month, being attacked again, he was admitted into this hospital, and the fever became arrested after 15 days' treatment with quinine. A fortnight after his discharge, the attacks recommenced; and he was admitted a second time. As he appeared to be labouring under an attack of remittent rather than of intermittent fever, he took a purgative on his admission, and was bled. This had the effect of changing the type of the disease into that of a regular intermittent—the cold stage lasting an hour and a half, and the hot stage eight hours and a half. No other medicine was administered, and to-day he was placed under my charge.

21st April. The patient took the carbonic acid in the usual way, yesterday, being the regular day of the accession. This supervened, but the attack was of shorter duration and of a milder form. The same treatment was pursued on the subsequent fever day, but no attack was experienced then, or afterwards. He left the hospital on the 26th.

Remarks.—In addition to fever, this man was suffering from congestion and hardness of the liver and spleen, with a yellow tinge of the skin—a common result with agricultural labourers in Italy. As these complaints were considered to be *incurable*, he was not allowed to remain in the hospital after the arrest of the fever. I treated him, subsequently, for these complaints, and he obtained almost entire relief before his departure from Rome, a fortnight after.

CASE 25. Francisco Napoleone, Æt. 38. 20th April, 1858. This patient was admitted on the 17th inst., having been attacked, six days before, with Tertian Ague for the first time. He took a purgative on the 18th, and had an attack of fever on the following day—the cold stage lasting an hour, and the hot stage eight hours.

April 22. The patient took the gas in the usual way yesterday, and had some slight symptoms of fever for an hour, without being preceded by chills or rigors. The same treatment was pursued on the 23rd, but no symptoms either of ague or of fever were observed. He left the hospital on the 26th.

CASE 26. Filipo Piermettei, Æt. 19. April 22nd, 1858. This patient, who was admitted yesterday, has suffered for *two years* from a Quartan ague, not regularly but at intervals; the disease ceasing for some weeks, and then returning again. He has been a patient in this hospital 5 or 6 times, the last time in the month of February; but the fever having returned ten days since, he was re-admitted. A purgative was administered yesterday, and to-day he was placed under my care. He was ordered to take the gas twice a day, on the days of the intermission; and in the ordinary way on the day of the attack.

April 24th. The patient took three doses of the gas yesterday, the paroxysm returning soon after the administration of the last dose. The attack lasted five hours, and was of nearly the same intensity as before.

April 27th. Four doses of the gas were administered yesterday, before the usual time of the accession, and the patient escaped without an attack—having only experienced some slight symptoms of fever for an hour. The same treatment was pursued on the following days, but no symptoms of fever having been experienced, he was discharged May the 3rd—cured. The bowels being confined, a dose of castor oil was ordered him the day before he left.

CASE 27. Francisco Cuilioni, Æt. 17. April 22. This patient, who has suffered from a Tertian Ague for 12 days, had an attack in January last, and, also, in September of the past year; but they were not of long continuance. At present, the cold stage lasts one hour, and the hot stage from 5 to 6 hours. He has taken no medicine at present.

April 24th. The patient commenced taking the gas yesterday, three hours before the usual time of the attack—one dose every hour. Shortly after the

3rd draught, the attack commenced, the duration of the cold stage being the same as before, but that of the febrile stage was much shorter. He was ordered to take two doses of the gas to-day, and three or four to-morrow, previously to the ordinary time of the accession.

April 25th. The patient escaped the cold stage altogether, but fever supervened and continued for two hours. To continue the medicine in the same way as before.

April 27th. The result was the same as on the last accession; fever having supervened, but being unaccompanied or preceded by the cold stage. As the pulse was strong and full, and the bowels confined, he was ordered a purgative, and the gas to be taken as before. On the next ordinary return of the paroxysm, the patient escaped with merely slight symptoms of fever for an hour—scarcely perceptible. Having had no return of the fever, the patient was discharged on the 4th of May.

Remarks.—This lad, I have reason to believe, did not take his medicine properly, not having been able to superintend the administration of it myself, as I usually did. It is not only necessary, that the patient should take the remedy at a proper time, but, also, in a proper manner, with the whole of the gas. If the latter escapes, the solution will be useless.

Case 28. Abile Stafanelli, Æt. 25. April 23rd.

This patient had an attack of Tertian Ague two months before, but it ceased at the end of six days, by appropriate treatment. The fever having returned nine days since, he sought admission into the hospital. The duration of the cold stage is one hour; that of the hot, seven hours. He was ordered to take the gas in the usual way.

24th April. The patient only took two doses of the medicine, the attack having commenced somewhat earlier than usual. The symptoms, however, were less intense than before, and the attack only lasted half the time.

27th. Four doses of the medicine were taken yesterday, and the patient escaped without any regular attack—merely slight symptoms of fever for half an hour.

29th. Having had no return of fever yesterday, he was ordered to be discharged.

With a few exceptions,* these were all the patients treated by me at this period; but, as this occurred in the Spring, when only old and rebellious cases are to be met with, or, else, recent and slight ones, I expressed a wish, on resigning my charge, to be enabled to resume these clinical investigations in the Autumn, when severe and malignant cases prevail. Having, on my return to Rome, renewed my application, I was informed by the Commendatore, or President of the Hospital, that my request could not be complied with. This led to a correspondence on the subject, which ended in an offer being made me to have some additional cases; but, as it was evident, that the whole medical staff were opposed to me, I declined the offer. These circumstances are, of course, uninteresting to the reader; and I only refer to them in order to show, that the fact of my not having had other and more severe cases to treat did not rest with me. I should have been ready to undertake the treatment of every severe case that entered the walls of the hospital, if allowed to do so. The above cases, however, were sufficient to show, that the fevers in Rome, and those previously treated by me in Spain, were produced by one and the self-same cause; and that cause the operation of the poison termed Malaria.

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* Observing the result, some of the patients imagined or were told, that the remedy was a very powerful one. They, therefore, refused to take it a second time: and as it was impossible to cut short the disease in every case, on the first administration of the medicine, I was obliged to turn these cases over to the Physicians of the hospital.

In order to exhibit the results obtained in the preceding cases more clearly, they have been collected together in a Tabular form: the last Table being copied from the report sent in by me to the President of the hospital, on resigning my charge.*

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* See Tables A and B, next page.

TABLE A.—CASES OF INTERMITTENT FEVER, TREATED IN SPAIN.

No.	Names.	Age.	Form of the Disease.	Duration of the Disease.	Duration of the Paroxysm.	Commencement of Treatment.	Number of attacks after.	Duration of Treatment.	Date of Discharge.
						1835.			
1	Rita Garcia	30	Quotidn.	10 months	5 hours	Feb. 6th	One	4 days	17th Feb.
2	Joaquim Gosalvez	12	,,	1 ,,	8 ,,	,, 12th	,,	5 ,,	19th ,,
3	Antonia Alvoreha	34	Tertian	6 ,,	13 ,,	,, 20th	,,	3 ,,	25th ,,
4	La Señora ——	30	,,	7 ,,	4½ ,,	,, 25th	None	3 ,,	2nd March
5	Juan Seldran	20	,,	1 week	3½ ,,	Aug. 25th	One	5 ,,	3rd Sept.
6	Francisco Yanez	40	,,	17 days	10 ,,	,, 28th	Two	6 ,,	7th ,,
7	Pedro Trebiño	50	Quotidn.	14 ,,	8 ,,	Sept. 15th	None	5 ,,	22nd ,,
8	Pedro Gonzalez	37	,,	30 ,,	2½ ,,	,, ,,	,,	4 ,,	,, ,,
9	Manuel Jolan	22	Tertian	3 ,,	4½ ,,	Aug. 21st	,,	4 ,,	9th ,,
10	Blas Adan	19	,,	11 months	8¼ ,,	Sept. 5th	,,	4 ,,	28th ,,
11	Manuel Estremeño	18	Quotidn.	4 days	6 ,,	,, 6th	Five	6 ,,	15th ,,
12	Pedro Gutierrez	19	Quartan	3 ,,		,, 15th	One	3 ,,	26th ,,

TABLE B.—CASES OF INTERMITTENT FEVER TREATED IN THE HOSPITAL OF THE HOLY SPIRIT, ROME.

	Names.	Age.	Form of the Disease.	Duration of the Disease.	Duration of the Paroxysm.	Commencement of Treatment.	Number of attacks after.	Duration of Treatment.	Date of discharge of Patient.
						1858			
13	Filipo Gentile......	20	Tertian	2 days	2½ hours	6th April	None	4 days	10th April
14	Pasquale Borgini	21	,,	,,	1½ ,,	7th ,,	,,	3 ,,	,, ,,
15	Luigi Frati	20	Quotidn.	3 days	5 ,,	,, ,,	One	3 ,,	14th ,,
16	Domino. Bicarelli	27	Tertian	2 months	12 ,,	8th ,,	None	4 ,,	12th ,,
17	Luigi Bartoli	13	,,	4 days	3 ,,	,, ,,	,,	3 ,,	12th ,,
18	Santolo Sartzoli	23	,,	3 ,,	9 ,,	,, ,,	One	5 ,,	13th ,,
19	Luigi Marinelli	22	Quotidn.	10 ,,	3 ,,	,, ,,	,,	6 ,,	15th ,,
20	Luigi Vincino..................	24	,,	10 ,,	6 ,,	14th ,,	,,	4 ,,	19th ,,
21	Natali Cochiotti...	11	Tertian	21 ,,	2½ ,,	,, ,,	None	4 ,,	,, ,,
22	Antonio Piacentini	13	,,	42 ,,	3 ,,	15th ,,	One	4 ,,	,, ,,
23	Pietro Capparucchi	22	Quotidn.	7 months	10 ,,	16th ,,	None	5 ,,	21st ,,
24	Filipo Cuchetti	33	Tertian	8 days	12 ,,	19th ,,	One	4 ,,	26th ,,
25	Francisco Napoleone	38	,,	6 ,,	9 ,,	20th ,,	None	4 ,,	26th ,,
26	Filipo Piermettei	19	Quotidn.	10 ,,	5 ,,	22nd ,,	One	10 ,,	3rd May
27	Franciso Cuilioni	17	Tertian	12 ,,	7 ,,	23rd ,,	,,	8 ,,	,, ,,
28	Abile Stafanelli	25	,,	9 ,,	8 ,,	,, ,,	None	5 ,,	29th April

Note.—These patients were visited every day by Dr. Gatti, the chief Physician of the Hospital.

Independently of the above, I had an opportunity of treating a case of Ague, which possesses great interest in a practical point of view, the disease having commenced on the west coast of Africa.

Lieut. M. C——, then in command of H.M. Steamer "Albert," was attacked, in June, 1843, with remittent fever, while his ship was undergoing repairs on the coast. He recovered from this, but had a relapse, in consequence of exposing himself too soon. Fortunately, the ship being then ready, he put to sea, to which circumstance this gentleman's recovery and life are no doubt to be attributed,—the attack being so severe, that he lay in a state of insensibility for several days. The remedies employed were calomel, purgatives, morphine, etc. While recovering, apparently, from this attack, Lieut. C—— was seized with intermittent fever, for which, on the arrival of the ship at Sierra Leone, he took Quinine and Calomel, under the advice of Dr. Fergusson. This treatment, and the supply of fresh provisions, sufficed to restore him to health and to strength, at the expiration of three weeks.

In the October following, after ascending the northern rivers, this Gentleman had another attack of what he calls intermittent fever, but which was rather remittent—the two Engineers, and one-third of the crew, being attacked at the same time. In April 1844, Lt. C. had another attack of remittent fever, which lasted three weeks. Shortly after this, the ship sprung a leak, rendering it necessary for her to go into harbour; when he was again attacked with regular intermittent fever, attended with delirium. Not yielding to the remedies employed, this long sufferer from the endemic fever of Africa was invalided and returned home. During the voyage, the attacks, which had previously been every day, became less frequent, and somewhat irregular in their form. No attack was experienced for three days after his arrival, but the fever then returned, and in a somewhat severer form; the cold stage lasting two hours, the febrile, one and a half, and attended, as before, with delirium. The patient was ordered Calomel and Quinine by his medical Attendant; but, as these remedies appeared to have no influence on the disease, he placed himself under my care.

The carbonic acid gas having been ordered for him, the patient took it in the usual way; that is to say, 4 doses shortly before the commencement of the accession. This returned, but the cold stage only lasted one hour instead of two: while the febrile stage, although of the same duration as before, was unattended with delirium. On the following day, the same treatment was pursued. The cold stage lasted only half an hour, and the febrile and sweating stage together three-quarters of an hour. No regular attack was experienced after this, only some slight symptoms of fever on one occasion:

and although nothing more was done, excepting to continue the draughts for a few days, this Gentleman experienced no return of the disease afterwards.

The result in this case is more particularly interesting by showing, that the treatment, which proved so efficacious in Spain and in Italy, is equally applicable to the same form of disease on the coast of Africa. If also my deductions be sound, as to the *modus operandi* of the remedy, we must infer, that the Agent productive of Ague is the same in all localities, and in all countries—in the torrid zone, as well as in temperate and cold regions. That such is the case, has been long ago inferred by all the best Writers on the subject; but we wanted some direct proof, in order to show, that the chemical composition of the poison is identical. Were it otherwise, some variation would be observed in the results obtained by the employment of the same chemical Agent or antidote: but there has really been none —at least as regards locality. The cure was as rapid with the old, rebellious, case from the coast of Africa, as with the comparatively mild cases in Spain and Italy. We may therefore conclude, with these facts before us, that carbonic acid gas is an antidote for the poison termed Malaria; and a specific for ague, if not for all the diseases produced by the operation of this almost universal Agent.

There is, in fact, no other way in which we can explain the *modus operandi* of the remedy. It is not an evacuant; it cannot act, therefore, by expelling the morbid matter out of the system. Nor is it a tonic, or a stimulant: if it has any action in the economy, it is that of a slight sedative. As such, it does not act indirectly, like quinine and other tonics, in producing the same ultimate result. Its action in the economy can only be explained in one way: this is, by concluding, that carbonic acid gas neutralizes the morbid matter productive of ague. When, also, we know, that all the forms of carbon have the property of neutralizing septic and other poisons out of the body, we have a right to infer, that it will produce the same result within, provided only that it has an affinity for the morbid matter. This we are justified in concluding is the case with the febrile matter productive of Ague; not only because we are unable to explain the effect of the remedy in any other way; but, also, on account of the rapid and immediate result obtained in many of the above cases. It will be seen, by a reference to these cases, that the effect has sometimes been almost *instantaneous;* as rapid, in fact, as when, after the ingestion of an acid into the stomach, a dose of magnesia has been immediately administered.

In stating my belief, that carbonic acid is an antidote to Malaria, and that, when introduced into the system, it is capable of neutralising the morbid matter then present in the blood, no objection, I presume, will, or can, be offered to such a conclusion. All doubts on the subject would appear to be solved by the experiments of Drs. Lawrence and Coates. It was deemed a curious subject of inquiry, whether artificial chemical changes can take place in the fluids, while they continue to circulate in living vessels; and the ordinary actions of life go on. With a view of ascertaining this point, these experimentalists commenced by throwing prussiate of potass into the cellular substance, and green sulphate of iron into the abdomen, in order to try, whether the well-known result of their admixture —prussian blue—would be produced. On performing this operation, they were gratified by observing a distinct and beautiful blue in the thoracic trunk and its contents, and in nearly *the whole substance and surface of the lungs.* It is, also, an important coincidence as regards the theory now broached, and the supposed union of the poison and the antidote in the centre of the circulating system, that when the above order was reversed, and the sulphate of iron, the substance of most difficult absorption, was injected into the cellular tissue, and the prussiate of potass, which is more readily absorbed, into the abdomen—where the process goes on with more facility—the chemical combination witnessed in the former instance did not take place—no trace of prussian blue having been discovered in the body.*

There is another circumstance well worthy of consideration, and which confirms the truth of the preceding conclusions. By a reference to the preceding Tables, it will be seen, that the result of the treatment is nearly the same in all—in those of the longest, as well as in those of the shortest duration. If there were any difference, it would appear to be in favor of the severest, the most rebellious, and the oldest cases. Such a result is an anomaly in medical treatment ; altogether different to that obtained under ordinary circumstances, and with ordinary remedies. These are most efficacious in the mildest, and the most recent cases; least so, in the most severe and the most protracted. Their efficacy, in fact, diminishes, in general, in exact ratio with the duration of the disease. It is also evident, from the facts before detailed, that the Antidote was as efficacious in those cases, in which Quinine and other remedies had failed, as in others. This is another proof, if it were wanted, that Quinine is not a specific in Ague; while it also shows, that carbonic acid gas is superior, in efficacy and in power, to all other remedies in the treatment of Ague.

Another great advantage is, that the gas is an innocuous remedy. Some antidotes are injurious *per se*, and can only be given in limited quantity on that account. Carbonic acid gas, on the contrary, can be taken in almost unlimited quantity, without producing any injurious result; it may, in fact, be said to be Nature's own remedy. Not only does carbon exist in every tissue of the body, but it is a component part of the blood itself; while its compound, or carbonic acid gas, circulates constantly in the veins. If the carbonic acid, given out during expiration, be formed in the systemic capillary system, as appears now to be a certain fact, the greater part,—all that not exhaled by the skin,—must traverse the venous system. Whether we are to look upon it, while there, simply as an extraneous substance; or, whether it performs an important function—that of combining with and neutralizing noxious substances, present in the blood,—we need not wait to inquire. It is sufficient for our present purpose to know, that this agent can be taken into the stomach in adequate quantity; be absorbed into the blood, and circulate there, without producing any injurious effect. As, however, great misapprehension exists in this respect, it being generally concluded, that carbonic acid is a poison, merely because it cannot be inhaled with impunity in large quantities; it may be as well, perhaps, to repeat the arguments and remarks, made by me on a previous occasion.

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* Richerand's Physiology by Copland, in Appendix, p. 613.

When atmospheric air is mixed with a certain quantity of carbonic acid gas, an individual who respires it, becomes asphyxiated and dies; an effect that is produced by every other gaseous body, excepting atmospheric air; with those that do not possess deleterious properties, the same as with those that do. But a similar effect is produced without the addition of another substance, merely by a variation in the normal proportions of atmospheric air; either by the addition of nitrogen, or by the subtraction of oxygen. Even oxygen itself cannot be inhaled in large quantities, and in a pure state, without producing mortal results. We ought not, therefore, to conclude, that carbonic acid gas is a poison, merely because an individual placed in such a medium dies, without other proof; any more than we have a right to infer, that water is a poison, because a person, plunged in this fluid, dies asphyxiated. That carbonic acid gas is not deleterious, *per se*, we may conclude, not only because it can be taken into the stomach in considerable quantities without inconvenience; but, also, from the fact, that it can be injected into the veins without danger, as the experiments of Nysten have already shown. It is only when more has been injected than the blood can absorb, that it occasions distension of the cavities of the heart and death—an effect that would be produced by atmospheric air, oxygen, or any other gas, injected in large quantities into the same vessels. It has also been shown, that carbonic acid gas can be injected into the carotid artery without occasioning any marked effect, Injected in larger quantities, it produces apoplexy, which appears to be entirely due, adds the above Experimentalist, to the abnormal distension of the capillaries of the brain.

M. Bernard has offered a satisfactory explanation of the injurious effects observed, when an individual is placed in an atmosphere highly charged with carbonic acid gas. He remarks: "After having heretofore regarded carbonic acid as a poison, we have now generally arrived at the opposite opinion. We can, in effect, inject it under the skin, into the veins, and into the arteries; and convince ourselves that this gas does not act as a poison. Why, then, does it cause animals that respire it, in a certain quantity, to die? . . . "There are three gases which are simply irrespirable, and which, incapable of supporting life, are nevertheless incapable, at the same time, of destroying it, in virtue of the properties which are peculiar to them. These gases are: azote, hydrogen, and carbonic acid gas. But the last-named gas, which resembles the others by its inaptitude to support life, differs from them by its greater relative solubility." Bearing this fact in mind, we may now give the explanation. "The blood which arrives in the lungs is venous blood, and, consequently, blood already charged with carbonic acid gas. In traversing the lungs, it (the blood) does not merely absorb the gases which it encounters, it also gives out a certain proportion of that which it contains; and the act cannot be regarded merely as an absorption—it is an exchange. Thus, in order that this exchange should take place, it is necessary that the gas escaping, and the gas entering, should be different in properties: and the presence of a certain proportion of carbonic acid gas, in the air which enters, brings it too near to the nature of the gas which escapes to allow of the exchange, excepting with great difficulty."* We may therefore conclude, that the injurious effects which arise, when this gas is inspired in certain quantities, are to be ascribed, not to its deleterious properties, but, simply, to its preventing the ingress of oxygen into the system, without which life cannot be maintained or preserved. But none of these effects are observed, when the gas is taken into the stomach. On the contrary, the addition of a certain quantity of carbonic acid gas to the venous blood, would rather favour the introduction of oxygen into the system, by the absorption of the latter on the escape of the former, during the act of inspiration. More than this, the gas can be inspired, when mixed with a certain proportion of atmospheric air, not only for hours, as I have myself proved by numerous experiments, but, also, for months and years, as is the case in certain localities, without danger, and with benefit to the health."* The epidemic cholera, in fact, has spared all those places where this gas exists in the atmosphere in larger proportions than usual—as the neighbourhood of mineral springs; while the ravages of this disease have been greatest in the small towns and villages, least in the large towns—there precisely where this gas is evolved in considerable quantities. In Jamaica, the mortality in the large towns varied from 10 to 20 per cent. of the population; in the small ones, from 20 to 40: and in the villages and settlements—the majority of them situated in hilly districts—the rate was 40, 50, 60, 70, 80, and 90 per cent. of the population. At an Estate called Batchelor's Hall, situated on the Plateau of a calcareous range of hills, and considered to be one of the healthiest spots in the Island, 70 out of 73 of the labourers were cut off, although a medical man was residing there at the time. It is also a fact, that the ravages of this disease have been least in the two Towns in which the population is the greatest, viz., London and Constantinople; although differing so greatly in every other respect. And yet, there are certain writers discoursing constantly of the poisonous effects of carbonic acid gas; and proclaiming, that it is the cause of disease and of death, although it happens to be the very Agent that preserves mankind from the one and from the other.

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* Leçons sur les effets des Substances Toxiques, pp. 138 and 9.

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* Vide Antidotal Treatment of the Epidemic Cholera ; Chapter, Prevention of the Disease.

Not only is carbonic acid gas an innocuous Agent of itself, but it possesses another advantage: it forms an innocuous compound with the poison that it neutralizes.* Some Antidotes are not only poisonous themselves, but they form injurious compounds with the matter that they neutralize. This cannot be the case with carbonic acid gas, and for the following reasons. Many of the patients, treated with this remedy, who were previously in a state of debility and of disease, not only lost the fever, but rapidly recovered their health and strength, without the employment of Tonics or any other remedy. We may therefore infer, that carbonic acid gas, not only neutralizes the malarious poison, but that it forms, when combined with the latter, an innocuous compound. More than this ought not to be expected, more cannot be obtained, by the employment of any Antidote, or of any remedy.

This is not all. In addition to being the most efficacious of all remedies in intermittent fever, carbonic acid is, at the same time, the cheapest and the most easily obtained, infinitely more so than that usually employed, viz., quinine. The expenditure for the last named drug, in those localities in which Ague prevails endemically, is enormous. Even in India, although the disease is not so prevalent as in the South of Europe, the cost to Government is something extraordinary. Mr. Turner, writing in 1861, says: "For the three Presidencies of Bengal, Madras and Bombay, the public (i.e., Government) expenditure of late years, in this one drug, reached the enormous sum of £50,000 annually."* Hence, a fear has been expressed that, if it continued to be employed to the same extent, the drug would hereafter become so scarce and so dear, as to limit its use to special cases only. Impressed, perhaps, with this conviction, a Circular, dated June 2, 1860, was issued by Deputy Inspector General Stovell to the Medical Officers in India, "to give an extended trial to the use of *Liq. Potassæ Arsenitis*, in doses and extent specified, in cases of *uncomplicated* intermittent fever." From a Report of Asst. Surgeon Chapple, it appears, that 140 men of the Royal Artillery, stationed at Poona, were thus treated. The treatment proved successful with 85 of this number; and unsuccessful with 55—being a ratio of 57·14 with the former, and of 39·28 with the latter. These were subsequently cured by the administration of quinine. This would be all very well, if the disease were really cured by the use of this drug: but this is not the fact. It is only the paroxysm that is arrested, in the great majority of cases. This, perhaps, is of more importance than the cost of the remedy.*

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* An Antidote, according to Orfila, ought to possess the following among other properties. 1st. It ought to be capable of being taken, in large doses, without any risk. 2dly. While acting on the poison, it ought to deprive it of all its deleterious properties.

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* Medical Times and Gazette, Sept. 28, 1861.

Having thus attempted to prove, that carbonic acid gas is a specific in Ague, the question naturally arises, will it be a specific, also, in all the other forms of fever, and in all the diseases produced by the same cause? If by specific be meant Antidote, there can be but one answer to the question—Yes. But if, by specific, be meant a remedy, that will cure all diseases, alone and without the assistance of any other, we must answer—No! An Antidote, it should be remembered, acts by removing the cause, or, in other words, neutralizing the poison: not by remedying the effects, that may have been produced by the presence of the morbid matter in the body. In some cases, the removal of the cause will be sufficient to remedy the effects; but, in others, such a result will not be, cannot be, obtained. Let us take, as an example, a case of accidental poisoning by vitriolic acid. If the acid be not too strong, and the quantity too large, the administration of an Alkali immediately will be sufficient to remedy all the effects, that may have been produced by the presence of the poison in the stomach. But, if a certain interval has elapsed, or, if the acid be very strong, the mere neutralization of the poison will not be sufficient to remedy the effects that may have arisen. Inflammation, ulceration, and gangrene, of the stomach and bowels will, probably, have supervened; and, at a later period, depression of the nervous system, convulsions, and coma. These morbid states will necessarily have to be remedied by special and appropriate means—the Antidote, or alkali, being insufficient to effect their removal. On the other hand, the alkali might be unable to combine with the acid, when administered, in consequence of the poison being absorbed into the blood, or having been transported to some part of the body to which the Antidote is unable to penetrate. We must expect similar results by the employment of an Antidote in the different forms of fever; more especially in the continued and remittent forms, which are distinguished from the intermittent by the circumstance, that there is no regular intermission —a circumstance of no small importance, as must be evident from what has gone before. It will be desirable, therefore, to ascertain what difference this circumstance makes in the treatment of these diseases.

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* The Russian Government gave an order, at the commencement of the present Turkish war, for the purchase of Quinine in Italy, to the amount of half-a-million of francs—£20,000. In consequence of this great demand, the drug became trebled in value, having risen from 6sh. to 16sh. the ounce. The Turks also bought *in one day*, from a wholesale house in Paris, 100 Kilogrammes of sulphate of Quinine.† This, at 16sh. the ounce, would amount to £1,200. The wholesale price of bi-carbonate of Soda is not more than 10*d.* a lb.; and although citric and tartaric acids are about three times as much, in England, lime and lemon juice can be obtained in warm climates—there precisely where fevers are most prevalent—for a mere trifle.

† British Medical Journal, July 7, 1877.

It has been shown, in the experiments before detailed, that the effect was slight, when the Antidote was administered in the febrile stage: although such speedy and almost instantaneous results were produced during the intermission. At this period, the Antidote, from its solubility and ready absorption into the venous system, is brought into immediate contact with the poison there present. But, in the cold stage, and, also, in the febrile stage, the Antidote is unable to reach the poison, at least in any quantity; in the one case, because absorption is arrested, and, in the other, in consequence of the gas passing out of the system with the expired air, instead of being carried on into the arterial system. It is impossible, therefore, that the same prompt and beneficial results should be obtained in continued and remittent fevers as in intermittent, by the employment of the same agent; unless it be resorted to at the very commencement of the attack, or, in the formative stage. When this is done, the febrile stage may generally be prevented, and the course of the disease be arrested. Not that the gas is useless in the febrile stage: on the contrary, it is more beneficial than any other known remedy. This may be ascribed to two circumstances. Although it has been inferred, that the gas usually passes out of the system with the expired air, a portion, doubtless, will be propelled into the arterial system: in which case, some of the poison, present in the systemic capillaries, would be neutralized. Then, again, although it has been concluded, that the greater part of the morbid matter is contained in the capillaries of the skin, during the febrile stage, a certain quantity will probably pass into the venous system with the flowing current, by regurgitation or otherwise. This inference is confirmed by the fact, that there is, even in continued fever, a slight, although scarcely perceptible, remission, in the course of the 24 hours. If this be regular, and if the Antidote be administered at this period, a certain portion of the poison would be constantly and daily neutralized, and its return to the capillaries of the skin be prevented. As such, the febrile symptoms would be lessened instead of being increased, as constantly occurs after these slight remissions. The result would, of course, be greater in remittent fever, in which the remission is more pronounced, and of longer duration. In this way, we shall be enabled to account for the benefit I have experienced by the employment of this agent, when given alone, and without any adjuvant, in cases of continued and remittent fever, even when presenting the purely sthenic form. But, as the benefit thus obtained is slow, and comparatively small, we must resort to those adjuvants that have a specific action in the economy. Such are the hydro-carbons, substances which, instead of passing out of the system with the expired air, are resolved into their constituent elements, become component parts of the blood, and are carried forward into the extreme divisions of the arterial system. Here the hydrogen, by its union with the oxygen, is converted into water, and the carbon into carbonic acid gas. If, therefore, the same processes go on during attacks of fever as in health, a certain quantity of carbonic acid gas will be formed in the system, and precisely in the situation where it is most required, viz. in the systemic capillaries. Hence the benefit that is derived by the use of fruit, vegetable acids, particularly when combined with sugar, and other substances containing carbon in excess. It is also probable, that benefit can be derived in another way—by the inhalation of the gas, and its introduction into the arterial system. This plan was adopted in a few of the cases treated by me in Spain, with the effect of preventing a return of the paroxysm. As, however, the febrile stage in ague is of short duration; and as the morbid matter, according to the conclusions before drawn, is principally contained in the venous capillaries; it is somewhat doubtful, whether the same result would be obtained in the purely sthenic form of continued fever, when the poison, as we have reason to believe, is shut up in the secerning capillaries. Not having had the opportunity of making the experiments myself, this problem remains to be solved.

As must be self evident, it would not be right, in the sthenic forms of fever, more particularly when attended by inflammation of a vital organ, to trust to the administration of the Antidote alone: those agents must be resorted to which tend to expel the poison out of the system. What these are have been already considered in the previous chapter. There are, however, other forms of continued fever besides the purely sthenic, or acute: those characterized by the opposite condition, or by depression and loss of vital power. Such are Yellow Fever, Typhus, etc. With these forms of fever, more can be effected, by the employment of an antidote, than with the former. This will be rendered evident by the facts adduced in the next Chapters, which are devoted to the treatment of these particular affections.

CHAPTER IV.

THE TREATMENT OF YELLOW FEVER.

The term Yellow Fever, usually employed to designate the epidemic fever of the West Indies, of the West Coast of Africa, and of certain latitudes of the American Continent—to which regions, with the exception of the southern coast of Spain, it has hitherto been confined—is not a proper designation. The yellowness of the skin is not peculiar to this disease: it is met with in the seasoning fevers of the West Indies, and, also, in the endemic fever of these countries. On the other hand, this symptom—yellowness of the skin—is not always present in attacks of yellow fever. The true, pathognomonic, symptom of Yellow fever is the *black vomit:* it is this, which distinguishes this disease from all others: although, like the yellowness, it is not invariably present. In such cases, which are generally fatal, the black fluid is invariably found, after death, in the stomach, thus showing that its non-rejection was caused by want of power. African Typhus, as it has been designated by some writers, or *le Typhus d'Amerique* as it is called by Dr. Bally and other French authors, is a more appropriate term.

CAUSE.—By the generality of writers, this disease is divided into two forms, the endemic and the epidemic—the former being referred to local causes, or to malaria; and the latter, to a specific cause, or to contagion. There would seem to be no sufficient reason for such a distinction, the morbid phenomena being the same at all periods; the only difference is in their intensity, and in the fact, that the disease prevails generally at one time, in certain localities only at another. Thus, at Vera Cruz, New Orleans, and other places, where yellow fever is endemic, strangers, who arrive there, not only in the sickly season but at other times, will be attacked with the disease in its severest form, at the very time, perhaps, that the inhabitants are entirely exempt. Take the following example, as related by Baron Humbolt. Two rich inhabitants of the city of Mexico—where yellow fever is unknown—arrived at Vera Cruz in the evening, in order to take the Packet the next day for Europe. Fearing to contract the disease, although it was not then prevailing in the town, they determined to remain in their carriage during the night, instead of going into the hotel. In the morning, they were attacked with "black vomit;" and, before the evening, both were numbered with the dead. Contagion, in such a case, is altogether out of the question. Besides, the same result is observed in uninhabited districts, hundreds, nay thousands, of instances of which have been recorded at different times. The only way in which we can account for such attacks is by supposing, that a poison exists in the air of these localities: similar results being constantly observed in all those situations in which malaria is known to be in operation. Nothing, in fact, is more common, in malarious districts, than for Strangers to be attacked with ague, although there may not be a single case, at the moment, among the inhabitants. If, therefore, sporadic cases of yellow fever be due to the operation of malaria, we may infer, that the cause is the same when it prevails epidemically: for like effects, according to an old axiom, must be produced by like causes. All we have to do is to conclude, when the disease spreads, that there is, not a variation of, but merely an increase in the intensity of, the operating cause, and hence its extension to other localities. As, however, this part of the subject has been discussed by me on a previous occasion, I shall assume, for the moment, that yellow fever, whether prevailing endemically or epidemically, is a product of Malaria.*

SYMPTOMS.—In the majority of cases of Yellow fever, the attack is ushered in by a well-marked cold stage, which is followed, at a longer or shorter interval, by the febrile stage, but this is seldom complete. On the contrary, the depression of the organic and vital functions, characteristic of the premonitory stage of continued fevers, and of the cold stage of ague, is seldom entirely removed. On the subsidence of the cold chills, there are generally severe pains in the orbits and forehead, as, also, in the loins and limbs. These are followed by a peculiar burning heat of the skin, flushed face, rapid pulse, glassy, suffused eyes, and, frequently, delirium. These effects are accompanied or followed by epigastric pains, costiveness, with nausea and vomiting of a dark-coloured or brownish fluid, resembling coffee-grounds—called the "black vomit." At the same time, a light, dark, or muddy, yellowness appears under the eyes, and gradually spreads over the whole body. Great anxiety, restlessness, and watchfulness, are generally present, with scanty or suppressed urine, followed by hiccough, and, in the majority of cases—by death. In other cases, again, and these are generally the most severe and the most fatal, there is no cold stage, no re-action, and scarcely any febrile symptoms; the patient being, in general, unconscious both of the cause of his illness and of his danger, until the black vomit appears, and apprises him of both. In fatal cases, the same dark grumous matter frequently escapes from the bowels; while hæmorrhage may take place from the eyes, nose, ears, and other outlets of the mucous canals.

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*Vide Epidemiology, Chapter I., The Doctrine of Contagion.

TREATMENT.—Like the majority of diseases, the treatment of this fever has hitherto been altogether empirical: and, consequently, unsuccessful—more so, perhaps, than that of any other disease, particularly when it appears in an epidemic form. Dr. Clarke, one of the earliest writers on the disease, and whose experience was great, states, he only recollects four patients who recovered, after "black vomit" had appeared. Dr. Riseuno declares that, in the visitation of yellow fever, at the commencement of the present century, in the south of Spain, 20,000 persons were cut off in Carthagena, out of a population of about 24,000 remaining in the city—10,000 having fled at the commencement of the outbreak. At Barcelona, in 1821, the mortality, according to Dr. Rocheaux, amounted to 19 out of every 20 attacked, at the commencement of the outbreak, but diminished to two-thirds towards the termination. In New York, also, in 1822, the proportion of deaths toward the close of the epidemic was as three to four—or 75 per cent. We may therefore conclude, that there were very few recoveries previously, the disease being, almost invariably, more fatal at the commencement of the outbreak. At Gibraltar, in 1828, only one recovered of the first 35 admitted into the Civil Hospital. Whether this excessive mortality is to be referred to the malignancy of the disease, or to its improper treatment, may admit of dispute. One thing, however, is certain; if the remedies formerly employed, for the treatment of this disease, did not kill the patient, they could hardly be expected to cure him. This will be evident by a reference to the treatment, that was then adopted.

Dr. Jackson, whose work was a kind of text-book, in the British Colonies, for many years, recommended bleeding and purgatives at the commencement of the attack; and a variety of remedies, or, rather, poisons, subsequently—calomel, turpentine, white and blue vitriol, sugar of lead and opium. As regards blood-letting, although some recoveries are recorded after this operation, the effect, in general, has been to hasten the fatal termination. Dr. Imray states, that when the pulse is strong and full, with burning skin, and the indications of blood-letting are present—as in young and plethoric subjects—relief is usually obtained: but it is not permanent. The short respite gives place, after one or two hours, to a renewal of the symptoms in an aggravated form; while, if the bleeding be repeated, the collapse and fatal termination are hastened.* Calomel is no less contra-indicated in this disease. As Dr. McCabe remarks, "the opposite principles of depletion and of stimulating were adopted by different Practitioners. The one party bled and purged, and kept the patient as low as possible, and continued to do so till the stomach had lost its tone, and the introduction of nourishment, to support the exhaustion of the system, became impossible. After the patient had lost 100 or more ounces of blood, and taken 200 or more grains of calomel, nourishment was allowed him; but, then, it was usually too late; the stomach rejected it."* In adition to this, calomel would appear to act, sometimes, as a direct poison; as was particularly observed during a visitation of yellow fever at Rochester (U. S.). This doubtless is to be referred to the fact, that the calomel was converted into corrosive sublimate by the muriatic acid in the stomach; a result that Dr. Prout considered to be not uncommon, when there is an excess of acid in this organ, as is generally the case with young children. The same state, according to Dr. Stevens, exists in yellow fever, as the matter of black vomit, not only reddens litmus paper, but effervesces freely with the alkaline carbonates. Hence, probably, the cause of the deleterious effect of calomel in this fever, The other preparations of mercury, if not as injurious, have been equally inefficacious. My old friend, Dr. Arejula, of Barcelona, who was in the midst of the pestilence, during the prevalence of yellow fever in the South of Spain, in 1804, stated, that a Physician, who was sent by the Cortes to render assistance, began by bleeding. He then had recourse to the internal exhibition of mercury, and to mercurial frictions; but, added Arejula, "he will not, I am very sure, publish the effects of this destructive treatment." That the exhibition of mercury is useless, if not injurious, would appear to be shown by a fact mentioned by Dr. McCabe, who says: "I have seen persons using mercury for the cure of other diseases; and, while ptyalism was present, attacked with fever, and die of it." After the calomel, with or without a purgative, ether, or an ounce of turpentine, was ordered; and then, to allay the irritability of the stomach, increased by the above treatment, sugar of lead was given. On the other hand, if there was no vomiting, an emetic of blue vitriol was substituted for the lead.* Referring to the employment of emetics, in yellow fever, Warren remarks: "Generally speaking, the effects of such an administration, in this distemper, are so dismal and surprising, that the stomach will receive neither medicine nor sustenance for the future; until, after the most laborious and painful concussions, hiccups, and almost convulsive contractions of the parts, signs of inward gangrene discover themselves, and nature's strength is at length quite spent, and worn away."†

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* Edin. Med. and Surg. Journal, Vol. 53.

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* Edin. Med. and Surg. Journal, Vol. 15, p. 538.

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* This heroic treatment is, what Jackson termed, "taking the business, as speedily as possible, totally out of the hands of nature," and, we may add, as effectually as possible. When he had brought his patients to the verge of the tomb, he attempted to "infuse life into the system," as he termed it, by the application of red-hot irons to the back of the neck ! !—On the Fevers of Jamaica.

† Treatise concerning the Malignant Fever of Barbados.

Independently of direct poisons, as sugar of lead—the employment of which can neither be explained nor justified—the administration of another empirical remedy—opium—has been, as we might *à priori* have concluded, no less fatal. Dr. Conolly states that, when Yellow Fever was raging in Philadelphia, in 1793, opium was found to be most injurious. "In some cases, where the chain of the disease appeared to have been broken by other remedies; and when the patients were in a fair way of recovery, a single dose of laudanum has hurried them suddenly into eternity."

As might be inferred, stimulants have been freely administered in this disease by certain Practitioners. That they are utterly powerless in severe cases, and that the vital powers are in a state of the greatest depression, is evident from the following circumstance. During a visitation of yellow fever at Trinidad, in 1817, *three* bottles of brandy, according to Dr. McCabe, have been given to a patient in less than 24 hours, and the same proportion continued for several days. This, in fact, would appear to have been the ordinary quantity with those Practitioners, who had faith in the efficacy of stimulants.

With such results before us, we can hardly fail to infer, that it would have been infinitely better to have left the case to nature, rather than run the risk of bringing disgrace on an Art, which professes to excel and to supersede Nature, if it does not despise her teaching. This is the opinion of other Writers, men who have had great experience in the treatment of Yellow Fever. Warren long since remarked, while alluding to this fever; "If the patient has the good fortune to recover from his illness, and out of their (the doctor's) hands, it is assuredly more owing to his own strength of nature, and happy temperament of body, than to their art or advice." Rush also stated, that "the history of the Yellow Fever, in the West Indies, proves the advantage of leaving patients to their own judgment:" and Lind added: "that a greater proportion of sailors, who had no physicians, recovered from that fever than of those who had the best medical assistance." Riseuno, a Spanish Physician, referring to the treatment of the disease during the visitation of Yellow Fever in the South of Spain, at the commencement of the present century, states, he was unable to fix on any plan, that could be depended on. And he adds: "the mildest, simplest, and easiest, method is that which is preferred by nature. This has been repeatedly observed, either in the case of those persons who, *being abandoned by their family and physician, are* unexpectedly *preserved;* whilst, on the other hand, many of those who were assisted by physicians, and who took and retained a great quantity of medicines, *expired under this treatment.*" * "A road, therefore," as Jackson, who had witnessed the failure of all his remedies, candidly confessed, "was thus left open, not only for improvement, but almost for total innovation."

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* Letter inserted in Sir William Burnett's Work on the Mediterranean Fever, p. 241.

Of late years, and since its successful employment in intermittent fever, Quinine has been very generally employed in the treatment of Yellow Fever, and its success vaunted by several writers. It is impossible, however, to conclude, that this drug can be of any real benefit in such a disease. If given in small doses, it would be inoperative, particularly in those cases which run their course in one, two, or three days. On the other hand, if administered in large doses, it would depress still more the vital forces, and hasten the fatal termination. Although more indicated, apparently, in milder cases—those which do not run their course so rapidly—the contrary would appear to be the fact. As it is precisely in these cases, that the febrile symptoms are more particularly manifested; and as the administration of quinine, in the febrile stage of ague, is not found to be attended with any marked effect; we may infer, that it would be equally inefficacious in those cases of Yellow Fever, characterized by febrile excitement, rather than by vital depression. This is Dr. Imray's opinion, who remarks: "No opportunity is afforded for the exhibition of quinine in the stage of excitement; while it has been found to be perfectly inert, even in the largest doses, when given in those cases attended with vital depression." These conclusions are strengthened by the result, that I witnessed, of some cases on board one of the W. I. Co.'s steamers, at St. Thomas'; and in which quinine was the principal remedy employed. The surgeon of the Ship was a great advocate for the employment of this drug, and had, in fact, written a pamphlet on the subject; but I could not perceive that it checked, in any way, the course of the disease—all the cases that I saw having proved fatal. It is clear, therefore, that a better and more specific mode of treatment is required for this disease, as well as for the majority of others.

If the deductions before drawn be correct, it will not be necessary to look far, in order to obtain a successful remedy. If both diseases be produced by the same cause, or the same poison; and if carbonic acid gas be a specific in ague, it will necessarily be so in yellow fever. We cannot expect to obtain the same speedy and invariable result in the one case as in the other, inasmuch as we shall seldom have an opportunity of administering the gas, in yellow fever, previously to the attack. There is a considerable difference, also, in the type of the two diseases; the one being a comparatively mild form of fever; the other, the severest and the most rapid of the whole class of fevers. Nevertheless, much more can be accomplished for such a disease, by the employment of an Antidote, than we might be disposed to allow on a first consideration of the question. As already shown, this disease is of a purely adynamic form—a result that may doubtless be referred to the great quantity of poison present in the blood; and its retention in the venous centres rather than in the external capillaries. Now it is precisely when the morbid matter is in this situation, that the best chance is afforded of bringing the antidote, on its introduction into the stomach, in contact with the poison. But the principal question is, have these inferences been put to the test of experience; and, if so, what has the result been?

Although inferring, after the result of my clinical experiments in ague, that carbonic acid gas would be an Antidote in yellow fever, it was not until 1850, that an opportunity was afforded me of administering the gas in this disease. During my visit to Jamaica in the above year, I was myself attacked with "black vomit," after a residence of some weeks at Green Island—one of the most malarious and pestiferous tracts in Jamaica. As there were no actual symptoms of fever, until shortly before I retired to rest, and thinking it to be merely an attack of ephemeral fever due to exposure, I determined to wait until the morning to see if the paroxysm would pass off. Soon after, however, vomiting came on, and observing the nature of the fluid thrown up, I immediately took an effervescing draught, and repeated it every half-hour, until 5 or 6 doses had been taken, and at longer intervals afterwards. The vomiting did not return, while the fever gradually subsided, leaving me, at the end of 24 hours, without any other symptom than that of debility. On my second visit to the West Indies, in 1854, I experienced a similar and more severe attack at Barbados, immediately on my return from St. Vincent, a more malarious island than the former. Feeling a great depression, —both mental and bodily—and being doubtful of the cause, I sent for Dr. Clarke, the principal practitioner in the island; but before his arrival, I was attacked with "black vomit." After looking at the contents of the hand-basin, and asking, with a shrug of the shoulders, what I had taken—an effervescing draught—Dr. Clarke left me without ordering any medicine, as I had expressed my intention to continue the remedy. It was only by an effort, almost of despair—so great was the prostration—that I continued the draughts during the night, being obliged not only to take them, but to instruct the nurse in the mode of mixing them. There was no return of the vomiting, and I felt myself out of all danger before the morning: but my convalescence was much slower than in the previous attack. Still, I took no other medicine, excepting an aperient.

This is all the evidence I have to offer on the subject. I may remark, however, that a favourite remedy in this disease, at the present time, is champagne. Dr. Imray states, that, "among the various stimulating substances which were had recourse to, few were found to answer so well as champagne." In the case of a Lady, the wife of an officer of the 74th, a dozen bottles of champagne, besides other stimulants, were used within a very few days. "In another instance of a Methodist clergyman, who had previously been accustomed to drink only water, the same treatment, in the last stage, evidently saved life. Even after bleeding at the gums had appeared, the effects of the champagne he described as being so soothing, that he invariably asked for it himself, when he felt its stimulating action wearing off, and the feeling of sinking returning."* That the spirit in the wine is of some use may be allowed, but, when we find that this stimulant is more beneficial than any other, although it contains a comparatively small quantity of spirit, we must ascribe the benefit to some ingredient not contained in other alcoholic drinks. Now the circumstance that distinguishes champagne from all other liquors is the presence of carbonic acid gas. That the benefit derived is to be ascribed to the presence of this Agent more particularly, may be inferred, not only from what has gone before, but, also, from the circumstance that, in my own attacks, I took neither wine nor spirit in combination with the draughts. Although this is all the direct evidence that has been obtained by me respecting the efficacy of carbonic acid gas in yellow fever, there is, fortunately, a considerable amount of indirect evidence; and this sometimes, as in legal cases, is of more importance than direct proof, being unsuspected.

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* On the fever in Dominica in 1838. Edin. Med. and Surg. Journal, vol. 53, p. 88.

Before adducing this evidence, it is necessary to state, that Dr. Stevens, some years since, recommended the employment of Salines, or the neutral salts, both in fever and in cholera. Starting on the well-known fact, that when the muriate of soda is added to venous blood, out of the body, the dark purple colour is changed to red; and observing that the blood in low adynamic forms of fever, and particularly in yellow fever, is of a darker colour than usual—the arterial blood sometimes resembling venous blood—Dr. Stevens inferred, that this dark, morbid, condition of the blood is caused, in great part, by the want of saline matter. He remarks: "The black colour of the blood is a certain proof of the entire loss, or, at least, of the great diminution, of the saline ingredients." More than this, Dr. Stevens infers, that the salts not only redden the colour of the blood, but add to its power of stimulating the heart. Hence, "in all varieties of malignant fever, the loss of the saline, or preservative power, appears to be, in every instance, the chief cause of the diseased condition, or entire dissolution, of the blood."* That the muriate of soda is a necessary ingredient of the blood is undoubted; but it is difficult to account for the loss of this or other salts in a disease, which comes on so rapidly; in which there are no alvine evacuations; and in which there is, in the severe and malignant cases, a suppression of the renal secretion. Then, again, in the sthenic form of fever, the blood, even in the veins, is of a bright rather than of a dark colour. According to Dr. Stevens' theory, there would thus be an excess of saline matter in the one case, and a deficiency in the other; but it is impossible to understand why there should be this variation, while no explanation has been offered of the cause of the phenomenon. Granting, however, that there is a loss of saline matter in yellow fever; and that the dark and diseased state of the blood is to be ascribed to this loss, we shall still have to search for the cause of the deficiency, as there can be no effect without a cause. There will be no occasion to look very far, Dr. Stevens having himself given us the clue. He observes: "This diseased state of the vital fluid is the effect of the remote cause acting on the vital current, but particularly by immediately lessening its vitality, and, *ultimately*, diminishing the quantity of its saline ingredients." (*Op. cit.*, p. 188.)

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* Observations on the healthy and diseased properties of the Blood, pp. 294, 357, and 364. London, 1832.

And again: "In the commencement of fever, the blood is dark from the effect of the poison; but, in the last stage, it appears to be black merely from the loss of its saline matter." (P. 230.) These conclusions are contradictory the one to the other. If the black colour of the blood, in the 1st stage of the disease, be due to the presence of a poison in the system, there can be no occasion to seek for another cause, in order to account for the same phenomenon in the last stage. It would be more philosophical to refer the increased blackness of the blood, in the last stage, to the continued operation of the primary cause rather than to another agent; more especially as there is no actual proof of the loss of saline matter in this disease. But the principal point for consideration is, not whether Dr. Stevens' theory be sound or unsound, but whether his treatment be beneficial or not; for it sometimes happens, that the treatment is right although the theory on which it is founded is false.

Dr. Stevens only gives the history of one Case treated by himself; and, even in this, he does not mention the actual remedies employed, merely stating that they were "the non-purgative saline medicines." This case was that of the Governor-General of the Danish Islands, who was attacked with yellow fever, during the epidemic at St. Thomas' in 1827. Having been bled and purged, without any benefit, and dangerous symptoms having supervened,—such as suppression of urine, no mention being made of "black vomit,"—Dr. Stevens determined to administer salines, in accordance with his preconceived theory. "In 24 hours afterwards, he was out of danger." Dr. George Stedman, it appears, adopted the same treatment; "and I can safely assert," says Dr. Stevens, "that after we commenced it, neither Dr. Stedman nor myself lost one case, where we were called (which we generally were) to the patients, within the first 24 hours after the attack." (Pp. 348—50.)

Such is the result that was obtained by the administration of salines, in the hands of Dr. Stevens and Dr. Stedman. When, however, the same remedies—the non-purgative, neutral salts—came to be employed by other Practitioners, they failed to perceive that any beneficial result was produced. "Notwithstanding the exhibition of these salts," remarks Dr. Imray, "in large doses, and the administration of the carbonate of soda, muriate of soda, chloride of soda, nitrate of potass, etc., variously combined, yet, *in no instance*, in which they were prescribed, could it be said, that they produced any marked effect, either in preventing malignant symptoms, or in removing them after they had made their appearance." (*Op. cit.*, p. 89.) Similar statements have been made by other writers, so that, as Dr. Copland puts it; "Dr. Stevens' theory, failing in support from observation and experience, has fallen, like an inverted cone."* How then is this discrepancy to be accounted for? There must be some cause for it, as we cannot suppose that the preceding writers were deceived, or that they have wilfully perverted the facts. A solution of the problem may probably be found, by referring to the employment of the same mode of treatment in the epidemic cholera, when precisely the same discrepancy occurred. As Dr. Stevens happened to be in London on the outbreak of the Cholera, in 1832; and his work, in which the neutral salts were proposed, for the cure of this rebellious disease, having just appeared; these remedies were adopted by a number of Practitioners, and, also, by Mr. Wakefield, at the Coldbath Fields Prison, under the direct supervision of Dr. Stevens. The result in the Prison was as follows.

According to the Report of Mr. Wakefield, there had been, up to that date, "nearly 100 cases, more or less evidently labouring under the influence of the cholera poison. Twenty-five of these assumed the malignant character of the disease."† Of this number, three died! Leaving the mild cases out of the question, being, in all probability, cases of diarrhœa, this would give a ratio of 12·50 per cent. for the cases of actual cholera—an unusually low rate. Although there is no account of so low a rate as this, by any other Practitioner, the employment of salines was extolled by many, and considered to be better than any other mode of treatment. By other writers, it was denounced as not only useless but detrimental. Observing this contradiction, the only conclusion to be drawn on the subject is, that there was some variation in the remedies employed, or, else, in the mode of their administration. It will be desirable, therefore, to ascertain what remedies were actually employed by Dr. Stevens at the Coldbath Fields Prison. They were as follows:—

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* Dict. Art. Pestilence Hœmagastric, paragr. 183.

† Medical Gazette, April 28, 1832.

1st. "The treatment was generally commenced with a *Seidlitz powder.*"

2ndly. "A powder, containing Carbonate of Soda (half a drachm), Muriate of Soda (a scruple), and Chlorate of Potass (7 grains), was given soon after the Seidlitz; and continued every 15 minutes, half hour, or hour, according to the urgency of the symptoms."

3rdly. "When the stomach was irritable (which is invariably the case in Cholera), the use of the above powder was occasionally suspended, and common *effervescing mixtures*, or small doses of the common *soda powders*, with an excess of the carbonates, were frequently used, until the irritation was lessened."

4thly. "*Seltzer water* was allowed *ad libitum*, when the patient expressed a desire for something to drink;" which he would most assuredly do, thirst being one of the most distressing symptoms in Cholera. (*Op. cit.*, pp. 458—60.)

It thus appears, that the patients treated by Dr. Stevens swallowed a large quantity of carbonic acid gas, compared with which the quantity of neutral salts administered was altogether insignificant. No wonder, then, that Dr. Stevens should say, when asked by Sir David Barry, on his official visit to the

Prison, where his cases of collapse were, that his treatment prevented the supervention of that stage of the disease: it having been conclusively shown by me, that patients, who have an unlimited supply of Seltzer water, never will fall into a state of collapse. But the curious part of the affair is, that the employment of this agent, so far from being in accordance with Dr. Stevens' theory, is in direct opposition to it. According to him, "an excess of this acid (carbonic) prevents the saline matter reddening the blood, when added to it." Such are the contradictions of certain medical writers; and such the variations between theory and practice.

Whether Dr. Stevens gave Soda and Seltzer water in yellow fever, we have no means of ascertaining. We only know, that he gave Seidlitz powders and carbonate of soda in combination with the neutral salts; the carbonic acid evolved from which might alone be sufficient to account for the success obtained in the treatment of this fever. This conclusion is confirmed by certain facts, adduced by Dr. Stevens, although for a different object—that of proving his own theory, not mine. He remarks:—"About the year 1795, Dr. Mitchell, of New York, brought forward, as new, an old theory, according to which fever is produced by an acid in the stomach: alkalies, or alkaline carbonates, were therefore recommended as the best means for effecting a cure. Dr. Baker, also, of Portland (U.S.), published a Paper, some years subsequently, in the "Medical Repository," in which he advocated the employment of the alkaline carbonates in fever; stating, "that he had scarcely lost a patient since he had resorted to them, three years previously. The fact of the success," adds Dr. Stevens, "was not doubted; but, as the result was in direct opposition to all the prevailing theories, it was considered as accidental; and such is the withering effects of blind attachment to preconceived opinions, that the generality of practitioners preferred to be unsuccessful with emetics, mercury, opium, brandy, etc., rather than adopt a practice, which had been proved, both by Dr. Baker and others, to be infinitely more successful than that which they were then using in the larger cities." (*Op. cit.*, pp. 324 and 5.)

Dr. Stevens also states, that "formerly, during the hot months, the marsh fever was nearly as fatal in some parts of the Genesee country as it is in Sierra Leone, or, perhaps, in any other part of the world. It is not so now; for, about seven years ago, a new method of treatment was adopted by some of the Physicians of Rochester, which has been uncommonly successful. This was not the result of any particular theory. It had been found, from long experience, that the old treatment with bark was of little use in the malignant form of that fever, and that the mere purgative practice was worse than none; while calomel, in some cases, acted with all the virulence of a poison. In consequence of this ill success, some of the Physicians resolved to abstain from employing any of the ordinary remedies in fever, and to wait and watch the result. During this period, some of them had observed, that *Soda and Seidlitz* powders were decidedly useful in that fever. This led them to inquire, whether it was the acids or the carbonates which produced the favourable result. The acids were tried, and found to produce the most destructive effects. They then tried the *alkaline carbonates*, and almost every one that was treated with these recovered.* From that period they have not used either emetics, calomel, acids, bark, or even the sulphate of quinine." . . . "Where there is great excitement they bleed in the beginning, and where the bowels are not sufficiently open they either combine a small quantity of rhubarb with the carbonate, or use a Seidlitz powder.

"Under this treatment the most malignant cases, which were formerly so fatal, are found now to be quite tractable; and almost the only fatal cases, which still occur, are in those unfortunate individuals, who fall into the hands of young practitioners, who, new fledged from the schools, and proud of their learning, brandish for a time the prescriptions of Rush, Chapman, Potter, etc., until their want of success, and the diminished number of their patients, compel them to adopt the more successful remedies used by the Practitioners in the backwoods, even in opposition to the famous dogmas taught *ex cathedrâ* by their most erudite Professors in the larger cities." It also appears that Dr. Stevens, when at Rochester (U.S.), in 1830, visited a number of cases of yellow fever with Dr. Henry. "During these visits, observing that, in every stage of the disease, he prescribed the carbonate of soda in large doses, I asked him on what principle he used that medicine so much: his answer was, that he trusted to this, almost entirely, not from any theory, but merely because he had found, from long experience, that this was decidedly the best remedy for curing the disease." (Pp. 320-22.) As there is evidently an abnormal quantity of acid in the stomach, in yellow fever, even the "black vomit" effervescing freely when the carbonate of soda is added to it, there can be little difficulty in accounting for the success of this remedy: provided only we have proof, that carbonic acid gas will cure the disease; and that the muriate of soda—the salt that would be formed by the union of the soda with the acid of the stomach—when administered alone is insufficient to do so. Evidence to this effect has been already advanced. It is unnecessary, therefore, to make use of other arguments in support of these conclusions; especially as more direct evidence will be adduced, in the next chapters, of the efficacy of carbonic acid gas in other forms of fever. It may be, and no doubt will be, said, as has been said before, it is impossible so simple a remedy can cure a disease of such malignancy. But, as Mosely has truly observed, "all (those) diseases that soonest destroy the frame, are most easily cured, when we have once found out the true method of treating them."*

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* It is difficult to understand why acids were given, as a crucial test, in this instance, as the acid, which is given with the seidlitz powder, is converted into a citrate or tartrate of soda before it is swallowed. On the other hand, we are not told, whether the *pure* alkalies were administered; they ought to have been, in order to enable the experimentalists to draw correct deductions on the subject. Had this been done, it would not, in all probability, have been left to the writer of these lines to point out the agent, that is so beneficial in these cases.

* On Tropical Diseases.

CHAPTER V.

THE TREATMENT OF TYPHUS FEVER.

By the generality of writers, this disease is supposed to be produced by over-crowding—Ochlesis—in ill-ventilated rooms, although this state is, in general, and with the population in the majority of towns and countries, a nearly constant one; while typhus only prevails epidemically in particular years, and, frequently, at long intervals. Typhus fever is, besides, the accompaniment of famine and destitution; but there is no reason to suppose, that there is more over-crowding at such a period than at other times. Then, again, typhus almost invariably follows in the track of armies in the field; although there can be no want of ventilation there, the troops being, in general, under canvas; and having too much rather than not enough of the external air. Having, however, been engendered by this or some other cause, its subsequent propagation is referred to infection, at least by the majority of practitioners in this country. If, however, this disease be propagated by contagion, why, we may ask, should it cease to spread in the summer, or when the hot weather arrives; typhus fever prevailing principally in the winter months, and in the spring? As it is impossible to account for this anomaly on the doctrine of contagion, we may conclude, that it is a false and illogical one. This circumstance—the prevalence of typhus at a particular time of the year—is, in fact, indicative of the presence of malaria, it being characteristic of this morbid agent, at all times and in all countries, to produce different effects at different seasons of the year.* I should therefore infer, that typhus fever is an endemic of cold and temperate climates—it being unknown in warm ones—the same as the severe, sthenic, form of continued fever is the endemic of intertropical regions. This inference receives support from another circumstance. If yellow fever be, as has been inferred, a product of malaria, typhus fever ought also to be referred to the same universal agent; for the former disease would appear to be merely an aggravated form of the latter. In order to show the analogy that exists between Typhus and Yellow Fever, it is only necessary to refer to some cases that occurred, during a visitation of typhus fever in Ireland, in 1826-27. "Patients," remarks Dr. Stokes, "who had precisely the symptoms of the general fever—whose symptoms presented nothing to draw particular attention to them more than to others—would be suddenly seized, about the 7th day, with extraordinary abdominal spasm—the spasms so severe, that they could be likened only to the worst cases of painter's colic. In the course of a very short space of time—I believe within an hour or less—it was observed, that the patients' face began to turn yellow, and a jaundiced tint rapidly spread over the whole body. . . . . "The horrible spasms continued for some hours. The patient then began to vomit 'black matter'—matter at first like coffee grounds, but, afterwards, quite black. In a few instances, he passed the same matter from the bowels, but, in most cases, the bowels were constipated."* Here then, we have the true pathognomonic symptom of yellow fever, in addition to the ordinary one—yellowness of the skin. The only difference observed was the production of spasm—an effect that may, probably, be referred to the fact of the attack being less rapid, and the vital powers, therefore, less depressed than in ordinary attacks of yellow fever, in warm climates. But the course of the disease, after it had commenced, was equally rapid and fatal. In some cases, death took place within *six* hours from the invasion of the spasms; in others, the patient lived for 24 or 36 hours. Of 17 patients attacked, only one recovered. And, lastly, it is mentioned, that the *post-mortem* appearances, observed in these cases, corresponded exactly with those that are found in true Yellow Fever, or African Typhus. It may be considered, that the cause productive of the disease is of no real importance, in a practical point of view, but it is otherwise: if typhus fever be due to the operation of malaria in the system, we shall know at once, which is the best and the most proper remedy to be employed for its cure and prevention.

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* By malaria, I do not mean—as is so generally the case in England—*miasm*, the product of the decomposition of animal and vegetable substances; but a gaseous and poisonous substance, which is extricated from the soil, at particular times and under particular circumstances, by well-known ascertained *laws*. Although the cause of its production is still a problem, we know that it is produced *beneath*, *not* above the surface; and that it is not a product of either animal or vegetable decomposition.

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* Lectures on Fever, p. 255.

SYMPTOMS.—The period of incubation, which is rather short in this disease, is characterized by symptoms common to the majority of fevers; such as, nausea, want of appetite, the mouth parched and dry, vitiated taste, languor, muscular debility, chills and rigors. These symptoms, in the majority of cases, and particularly in the severe form of the disease, are followed by increase of heat on the surface, particularly in the evening, or during the night; thirst, diminished secretion, quickened pulse—which is soft, easily compressed, and often irregular—and more or less derangement of the functions of the brain. The preceding, which may be said to constitute the first stage of the disease, is succeeded, from the 6th to the 12th day, by an eruption known as the typhus, or mulberry, rash; severe headache; increased heat and dryness of the skin; continual thirst; great muscular debility, with tendency to syncope in the erect position; somnolence and delirium: heaviness of the eyes, effusion of the conjunctiva, and aversion to the light. The patient assumes the supine position, and appears to be lost to all surrounding objects. Third stage. When no relief has been afforded, the lips, mouth, teeth, and tongue, are covered with a hard, brown fur; the tongue is parched; the secretions become still more vitiated, the urine dark and fetid, and the fœces highly offensive. The delirium is now persistent, being either noisy and violent (typhomania), or, more frequently, low and muttering; and, occasionally, partakes of the character of *delirium tremens*. The somnolence degenerates into coma; *petechiæ* appear on different parts of the body; blood is poured out from the gums, mouth, and nostrils; fetid and cadaverous sweats break out; the extremities become cold; while spasmodic twitchings of the tendons and muscles, hiccup, and intermitting pulse, indicate the final catastrophe.

When Typhus appears in the summer months, there is a greater tendency to febrile excitement, or re-action; but in the autumn or winter, the tendency is to putrescency; generally speaking, there are morning and evening exacerbations, preceded by rigor.

Complications. Bronchitis is the most common; and, next to this, pneumonia. When this is severe, gangrene of the lung sometimes follows.

Pathology. With the exception of the lungs, no particular lesion in any part of the body, or in any organ, is discovered, on the examination of the bodies of persons who have died of typhus fever. The only morbid effect observed is that of general congestion; not only of the internal organs, but, also, of the mucous membranes. Hence the effusion on the brain, so frequently observed; and hence, also, in all probability, the bronchial and other affections of the lungs, which appear to partake more of a congestive than of an inflammatory character.

If we analyse the symptoms, we can hardly fail to infer, that Typhus is a purely adynamic, or congestive, form of fever, not an acute febrile affection; an inference that is confirmed by the pathological results just described. Hence, Typhus has been termed *fièvre congestive* by certain French writers. If, therefore, this disease be produced, as has been inferred, by the presence of a morbid and extraneous substance in the system, we must also infer, that the principal part of the poison is contained in the venous system—an important conclusion as we shall presently find.

TREATMENT.—It is unnecessary to enter into a consideration of the various remedies, and the different modes of treatment, that have been adopted for the cure of this disease, after the remarks made in the introductory chapter. "It is apparent," as Dr. Tweedie justly observes, "that fever, when once developed, can rarely be arrested; or, in other words, that the means of expelling the poison, or of depriving it of its noxious effects, have yet to be discovered. The duty of the practitioner, therefore, is to endeavour to guide the disease, and to prevent, as much as possible, injury to organs essential to life; bearing in mind, that this requires a certain, and, probably, definite time to accomplish, even under the most judicious treatment."* But the majority of Practitioners have been unwilling to follow this wise course, or to adopt the maxims of Nature; they have attempted to "cut short" the disease, as the term is, and have only rendered the case more intractable than before. Referring to the treatment of this disease, Dr. Stokes remarks: "The attempts to cut short the disease by purgatives, blood-lettings, emetics, diaphoretics, etc., were not only futile but harmful—futile in this, that they did not arrest the fever, and harmful because their effect too often was, in ordinary words, *to spoil the case.* Copious sweating or venesection produced debility, and purgatives pre-disposed to dangerous disease of the digestive tube." (*Op. cit.* p. 323.) Others, impelled by these failures, have resorted to empirical remedies, or else have trusted, as a *dernier ressort*, to stimulants alone. It is not surprising, therefore, to hear the preceding writer—Dr. Stokes—exclaim: "We know of no cure for fever: *no man has ever cured it.* It is, however, curable spontaneously:" (p. 84) but only in a certain and limited number of cases. If, however, typhus be a product of malaria; and if carbonic acid gas be a specific for ague, it will also be a specific for typhus fever, both diseases being produced by the same poison. The result may not be, and cannot be, in fact, the same, ague being a mild, and typhus a severe, form of fever. Still, if we possess an antidote for the poison productive of typhus, we shall not only be enabled to treat the disease scientifically, but there will be a greater chance of curing it with certainty and rapidity, than by any other method, or by any other known remedy. It only remains, therefore, to ascertain, whether carbonic acid gas has been employed in this disease; and, if so, what the result has been. Having been accustomed to administer this remedy, for a great many years, in every case of fever that presented itself, I had collected the notes of a certain number of these cases; but they, together with all my MSS. and papers, were destroyed, in the fire at the Pantechnicon, a few years since. As such, I could only give the general result, without those particulars that are generally required to prove the efficacy of new modes of treatment. Fortunately, a certain amount of evidence can be obtained from other sources, amply sufficient to prove the truth of my own conclusions, viz., that carbonic acid gas is an Antidote in Typhus fever; and a certain remedy, if administered in the early stages of the disease.

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* Lectures on Fever, 1862, p. 211.

It having been ascertained, soon after its first separation from the alkaline carbonates, by Black, that carbonic acid gas, or fixed air, as it was then termed, possessed antiseptic properties, it was employed, by several practitioners, in those diseases presenting a putrid type. Dobson was the first to prescribe it, in what were then termed putrid fevers; but which were merely severe, or malignant, forms of Typhus. The following cases, given by this writer, will show what the result was.

Case 1. Mary Rainford, æt. 15, was admitted into the Liverpool hospital, in 1773, labouring under an attack of fever. She had an emetic, and a purgative subsequently; took the *spiritus minder*, and lemonade as ordinary drink, etc., etc., but the fever increased in intensity, and, "on the 6th day, large *petechiæ* spread over the body, the tongue was covered with a brown fur, and the teeth with a fur of a blackish colour; she was very feeble, got no sleep, and was frequently delirious, especially during the night. Pulse 135, and weak.

"She was ordered a scruple of Salt of Tartar (carbonate of potash) dissolved in half an ounce of water, with half an ounce of lemon juice, to be taken in a state of effervescence, and to be repeated every hour. The patient took no other medicine; the symptoms became more favourable, and she was out of danger in four days."

Case 2. Alice Rigby, while under treatment, in the same hospital, for a sore leg, was attacked with fever, and treated with the ordinary remedies; but without success. "On the 7th day, *petechiæ* appearing on the body, and the patient's brain being much affected, the effervescing draughts were administered, the same as in the other case. The *petechiæ* soon began to disappear, the pulse became fuller and slower, and the fever was, in 6 days, entirely removed."

Case 3. Ann Knowles, who had been in the hospital for some time, suffering from rheumatism, was attacked with the same or a similar fever. For this, she had taken ordinary remedies for 7 days, but continued to grow worse. "The pulse was weak and frequent; there was stupor, with a muttering kind of delirium; the whole body was covered with small *petechiæ*, and the stools, which were frequent, were highly offensive."

The draughts were ordered to be taken the same as in the other case; the bad and dangerous symptoms gradually subsided: and, on the 6th day, the fever had entirely left her. "She took no other medicine, and had no relapse."*

A case, narrated by M. Becu, a Physician at Lille, is no less interesting and instructive. Being desired to visit a workman, named *Alexander Dumartier*, labouring under a putrid fever, on the 27th May, 1784, M. Becu remarks, "On arriving, I found the patient delirious, and in a state of singular prostration; the eyes dull and cloudy; the countenance changed; the tongue black and coated; the respiration troubled; the pulse weak, small and trembling; the abdomen tympanitic, and of an enormous size; the inferior extremities cold and œdematous; and the body exhaling a cadaverous odour." The patient had been bled and purged, without any beneficial effect, and M. Becu ordered him an emetic, but the symptoms only became more alarming. "The stools were black, mixed with blood, fetid, and highly offensive, the urine not being much better; the patient appeared to be nearly suffocated, and breathing with difficulty; a cold, clammy, cadaverous, perspiration covered his body; and the pulse was scarcely perceptible. The idea then struck me to try the effect of water impregnated with fixed air. The remedy produced an immediate effect, and surpassed my expectation. Each time that the patient took it, he expelled flatus of the most horrid and insupportable smell. The meteorism of the abdomen became diminished; the respiration freer, and the urine flowed more abundantly." Bark, wine, and nourishing diet, were then ordered, and the patient gradually recovered.*

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* Medical Commentary on Fixed Air, 1779.

The gas was also recommended, in the form of clysters, by Dr. Priestley: and Mr. Hey, of Leeds, employed it, in this way, in a dangerous case of putrid fever, and with complete success. This method was also adopted by Dr. Warren of Taunton, and, in the following interesting case, among others.

"M. C—, aged 23, had laboured under an irregular nervous fever for ten weeks, at the end of which time symptoms of putridity made their appearance. The Peruvian bark, lemonade, and Port wine, mixed with Pyrmont water, were administered; but the symptoms increased, with fœtor of the breath, and bloody stools of the most offensive kind. Universal languors, with almost total insensibility, now supervened; an earthly coldness diffused itself through every part of his body; nor were the hottest fomentations, though continued three hours together, capable of procuring him any degree of warmth. Every breath he drew seemed to be his last.........I now ordered him clysters of *fixed air*, of which a large bladder full, containing near two quarts of fixed air, was every three or four hours injected; and his bark boluses were again given to him, as often as his stomach would allow him to take them. In the space of 18 hours, the cadaverous fœtor, arising from him, began to abate; large *vibices*, or putrid blotches, were now for the first time discovered on almost every part of the body; his pulse, however, was better, and his warmth, in some little degree, returned. The boluses and clysters were ordered to be continued. In four or five days the noisome smell became imperceptible, the vibices gradually diminished, and his fever left him. He is now perfectly recovered, and a living miracle of what 'fixed air,' under divine Providence, is capable of effecting on the human economy, in cases of the worst and most putrefactive nature."*

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* Journal de Medécine, Chirurgie, etc., T. 63, p. 490.

In other instances, and by other practitioners, the gas was given by inspiration.

Dr. Rotherham, of Newcastle, in a letter to Dr. Percival, referring to the prevalence of a severe form of fever in that town, says: "The putrid symptoms appearing, I gave moderate quantities of Port wine, and, in some cases, plentiful draughts of bottled cyder. But I certainly found the greatest benefit from the liberal use of factitious air, by *inhalation*.†.........When I have been called in the first stage of the fever, I think," adds the Writer, "I have not once failed, by persevering in this method, of keeping off every putrid appearance through the whole course.........But I have still something stronger to add: in some very recent cases, where I have not been called till the 11th or 12th day, and after the patient had been above eight days delirious; with the black, foul, crust of the tongue, fœtid breath, total insensibility of stool or urine; livid countenance, and almost every dreadful putrid appearance, by six hours unremitted application of these fumes, and sometimes washing the mouth with port wine, large sloughs have been cast off, frequently mixed with blood; the mouth and fauces rendered fresh as a rose, the breath perfectly sweet; and, in short, the putrescency totally vanished. I assure you, dear Sir, I do not magnify; but am happy in having several full and living proofs to produce.‡"

The carbonic acid was administered in another form, by the Rev. E. Cartwright, towards the close of the last century, with some of his poor parishioners at Bampton, near Chesterfield.

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* Experiments and Observations on Different Kinds of Air. By J. Priestley, LL.D., vol. 2, p. 377.

† The plan, adopted by Dr. Rotherham, was to place a vessel, containing some chalk and vitriolic acid in solution, alongside, and close to the head of, the patient; and thus allow him to inhale the gas for a certain time as it escaped. If well enough to sit up, the pan was placed on the bed, directly under the patient's mouth.

‡ Essays: philosophical, medical, and experimental. By Thos. Percival, M.D. 1776.

The first case was a boy, 14 years old, who had been ill some days with putrid fever, which was then prevailing epidemically in the village. "I then," adds Mr. Cartwright, "administered bark, wine, and such other remedies as my books directed. My exertions, however, were of no avail: his disorder grew every day more intractable and malignant, so that, for more than a week, I was in hourly expectation of his dissolution. . . . . While I was in conversation on this distressing subject with his mother, I observed, in a corner of the cottage, a small tub of wort working. The sight brought to my recollection an experiment, I had somewhere met with, of a piece of stale meat being made sweet by being suspended over a tub of wort, in the like act of fermentation. The idea instantly flashed upon my mind, that yeast might possibly be of service to my patient. Without a moment's pause, or reflection, I gave him two large spoonsful; and told the mother, if she found him no worse for what I had given him, to repeat the dose every three hours."* To the surprise of Mr. Cartwright, the patient gradually recovered by the end of a fortnight. Encouraged by this success, Mr. Cartwright administered the same remedy to about 50 other patients, in the course of two years, and adds, that he did not lose one.

The Writer adds, however, that he was generally consulted at an early period of the attack: but this was not always the case, as the following account will show. One of the Reverend gentleman's domestics having been taken ill, the Apothecary was sent for. "His complaint was a fever, which, in its progress, became putrid. Having great reliance, and I believe with reason, on the apothecary's penetration and judgment, the man was left solely to his management. His disorder kept daily gaining ground, till, at length, the apothecary considered him in very great danger. At last, finding every effort to be of service to him baffled, he told me he considered it as a lost case, and that, in his opinion, the man could not survive 24 hours. On the Apothecary thus giving him up, I determined to try the effects of yeast." Fifteen minutes after taking the yeast, a slight amendment was perceived in the patient; and he rapidly recovered during its continuance. Some bark and wine were administered at the same time; and he was allowed sago as soon as an amelioration in his symptoms was observed. Another no less interesting case was this. "Riding past a detached farm-house, at the outskirts of the village," says Mr. Cartwright, "I observed a farmer's daughter standing at the door, apparently in great distress. On inquiring into the cause, she told me her father was dying. I dismounted, and went into the house to see him. I found him in the last stage of a putrid fever; his tongue was black, a sanious ichor was oozing out of the corners of his mouth, his pulse was scarcely perceptible, and he lay stretched out, like a corpse, in a state of drowsy insensibility. I immediately procured some yeast, ... and poured it down his throat. I then left him with little hope of his recovery. I returned in about an hour and found him perfectly sensible, and able to converse." By a continuation of the same remedial Agent, he recovered rapidly and perfectly.

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* Beddoes. Letters &c. on the Medicinal Use of Factitious Airs.

Dr. Stokes, in a work on Fever, published in 1816, states, that he administered yeast, in Typhus fever, rather extensively; and he speaks highly of its efficacy, "in cases accompanied with symptoms of putrescency; and as being well suited to those with purple extremities, and gangrenous sloughing." Dr. Tweedie also remarks: "From the trials I have made of yeast, I am satisfied, that it is a remedy well suited to the low form of Typhus fever, and deserving of more general employment" (*op. cit.* p. 268). If, however, yeast be efficacious in the cases referred to by these two writers; and which, in fact, are the worst forms of typhus, it will necessarily be applicable to all cases, and to all forms of the disease.

We hear nothing more of this remedy until 1828, when its employment was advocated by Dr. Clanny, and for the following reasons. In 1818, Dr. Clanny applied himself to the task of investigating the proximate cause of Typhus; for which purpose he made repeated analyses of the blood in this disease, and also of healthy blood. The result was, that the free carbonic acid gas, always existing in healthy blood—to the extent of one cubic inch in every 16 ounces, according to this writer—is not found in the advanced stage of cases of Typhus, nor in any quantity after the 6th day.* Dr. Clanny was, therefore, induced to administer carbonic acid gas, in order to make good the deficiency which appears to exist in this disease: more especially as he ascribes some important functions to the presence of this gas in the system. As regards its employment, Dr. Clanny says: "The stomach ought at all times to be charged with suitable doses of carbonic acid gas, in the form of carbonic acid water, or of effervescing draughts, prepared from carbonate of soda and lemon juice in the usual manner, which may be given every hour or two, night and day, according to circumstances; whilst, at the same time, *enemata* of carbonic acid, in an *unmixed* (or pure) state, should be carefully administered as often as the case may require. . . . The quantity of carbonic acid, as well as the suitableness of the time for its administration, as an enema, must depend upon circumstances. . . . . It is best to begin with small portions of gas, say two or three ounces, and augment gradually." Dr. Clanny does not give the actual result of this treatment, or the histories of any of the cases treated by him. He merely says: "If my experience does not deceive me, medical men, who are called in early to attend typhus cases, should they understand the true proximate cause of this disease, as now for the first time pointed out, will be able (except when organic disease of the brain, lungs, or liver, prevails) to effect cures as readily as in intermittent fever, or in syphilis."* This is entirely in accordance with my own experience, never having met with a case of Typhus fever, in which, when the remedy was administered at an early period of the attack, the course of the disease has not been arrested. The only difference between Dr. Clanny and myself is the peculiarity of the theory he formed, respecting the cause of Typhus fever—a theory that can be explained in a few words.

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* Edin. Med. and Surg. Journal, Vol. 30, p. 221, An. 1828.

According to this writer, the blood, in advanced cases of Typhus, has only $\frac{75}{1,000}$ of albumen, instead of $\frac{121}{1,000}$; and only $\frac{22}{1,000}$ of fibrin instead of $\frac{28}{1,000}$, in addition to the loss of carbonic acid gas. "From these facts," adds Dr. Clanny, "I have come to the conclusion, that the *proximate* cause of typhus fever is a cessation of chylification, and, consequently, of sanguification." That the diminution in the ordinary ingredients of the blood, is to be referred to a suspension, partial or complete, of the digestive functions may be allowed; but, then, this suspension of chylification must be referred to some antecedent cause, which Dr. Clanny ignores. Then, again, the diminution of carbonic acid gas in the blood, may probably be ascribed, not to the absence of carbon in the blood, but to its non-production in the systemic capillaries, due to an arrest of the functions of these vessels. This, in truth, is the inference that I should myself draw on the subject. It matters not, however, in a practical point of view, whether the benefit derived by the administration of carbonic acid gas, is to be ascribed to the introduction into the system of, what Dr. Clanny terms, "the main spring of life;" or whether it be produced by the neutralisation of a morbid agent, present in the blood; it is the result with which we are most concerned.

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* Lancet, Sept. 19, 1829.

Independently of the gaseous form of the remedy, the alkaline carbonates have been administered in this disease, the same as in yellow fever, and their employment advocated by several writers. But as, after the preceding facts, little doubt can exist, that the benefit derived, in these instances, is to be referred to the decomposition of the carbonate in the stomach,—the liberation of the gas, and its absorption into the blood—rather than to the neutral salt or the alkaline base, it follows that little dependence can be placed on these agents unless an acid be present in the stomach. This can hardly be expected at a late period of the attack, when the secretions are either partially or entirely arrested: more especially if an alkali has been previously administered. The employment, therefore, of the alkaline carbonates, alone and uncombined with the gas, should be restricted to the early period of the attack, or the first stage of the disease.

If, as has been before inferred, typhus fever be merely a mild form of plague; and if the latter be only a severe form of intermittent fever; it follows, as a matter of course, that the agent which is an antidote in the two milder forms, will also be an antidote in the severe form of the same complaint. The conclusion is self-evident.

There is another disease, closely allied to typhus, if it be not produced by the same cause: this is Typhoid, or enteric, fever, which, on account of its importance, will require a separate consideration.

CHAPTER VI.

THE TREATMENT OF TYPHOID FEVER.

THIS disease, sometimes termed *enteric* fever, and, occasionally, gastric fever, is, like the epidemic cholera, a new one, having been unknown until the commencement of the present century. It was first recognised and described by M. Bretonneau in 1813. Dr. Trousseau, after stating that various Authors, as Broussais, Petit, Serres, Andral, etc., had described the different kinds of inflammation of the intestinal tube—under the terms gastrite, enterite, erythematous, erysipelatous, apthous, pustular, etc.—added: "None of the above Writers have referred these effects to a specific lesion. But the long and useful labours of Dr. Bretonneau have, at last, cleared up this question. Since 1813, he has collected a great number of facts, not only in his private practice, but in the hospital at Tours, at the head of which his ability has placed him. He has been led to distinguish a disease, the seat of which appears to be exclusively in the glands of Peyer and of Brunner, which are met with in the jejunum, the ileum, and the large intestines. He has given to this affection the name of *dothinenterie* or dothinenterite (δοθιην, bouton, pustule, and εντερον intestine); and he has explained the analogy; traced the symptoms; described with precision the variations, each day, of the disease: and he has brought together, so perfectly, all the elements of the diagnosis, that there are few of his students, or of those who, in great numbers, have become acquainted with his researches and his ideas, who, in the generality of cases, cannot easily distinguish this *now common form* of complaint from every other."* M. Louis, in 1829, published a Treatise on the same subject: and in this more detailed work, the symptoms, the diagnosis, and the pathology, of Typhoid fever, and its relation to other diseases, were clearly and accurately defined.† It is this Writer, who gave the name, Typhoid, to this particular affection. Although defined so many years before, it was not recognised in England until 1827, or in Scotland, until 1848. In America, it was first observed in 1833, and was described by Dr. Jackson, in a communication to the Massachusetts Society, in 1838.

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* Archives Générales de Médécine, T. x., p. 68. 1826.

† Recherches anatomiques, pathologiques et therapeutiques, sur la maladie connue sous les noms de gastro-enterite, adynamique, ataxique, typhöide, etc.

Typhus fever being at that period almost unknown in France, M. Louis could only compare it with the other fevers, and with the acute diseases, then prevalent in that country; and he separated it from the whole of them. But Dr. Gerhard, of Philadelphia, in a Memoir, published in 1837, drew this important conclusion: "that there may exist, and does exist, in fact, simultaneously in the same country, two diseases, that can be perfectly diagnosed; and in which it is easy to predict, during the life of the patient, the lesions that will be found, after death: these are typhoid fever and typhus, properly so called."* And M. Valleix, in a series of Papers, published in 1839, on the Typhus and Typhoid fevers of England, remarked: "these investigations (of Dr. Gerhard) are entirely in accord with those that I had previously made; and have led this distinguished observer to the same conclusion as myself: viz. that typhus and typhoid fever are *distinct diseases*."† Hence M. Louis, in the 2nd Edition of his Work, published in 1841, expressly stated; "that the Typhus fever of the

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* American Journal of the Medical Sciences, February and August, Nos. 28 and 40, 1837.

† Archives Générales de Médécine, Nov. 1839, p. 283.

It is a singular circumstance, that, previously to 1830, English medical men went over to France to study Typhoid fever; while French Practitioners came to England to study Typhus fever. It is no less singular, that typhus should now appear to be gradually on the decline in England; while typhoid fever is as regularly on the increase.

English is necessarily a very different disease to that which now engages our attention." (Vol. 2, p. 318.) Dr. Bartlett, the year after, expressed the opinion, that "the two affections (Typhus and Typhoid fever) constituted radically dissimilar fevers." And he adds: "To Dr. Gerhard and Dr. Pennock belongs the credit of having first fully pointed out, and clearly established, the most prominent and essential points of dissemblance between the two diseases."* In 1850, Sir William Jenner also published a Pamphlet, in order to prove, that Typhus and Typhoid fevers are distinct diseases:† and Dr. Tweedie, in 1862, drew the same conclusion, in his work "Lectures on Continued Fevers." Although written, at this late period, and although Dr. Tweedie enjoyed such great advantages as Physician to the London Fever hospital, it may be remarked, that a great many of his facts, respecting typhoid fever, have been taken from the work of M. Louis, which still remains the best monograph, perhaps, that has been written on the subject.

Cause.—By the generality of English writers, Typhoid fever is considered to be a product of the decomposition of organic matter, and particularly of that which forms the excreta of man. Hence it has been termed pythogenic fever, i.e. arising from putrescency. As it has been my object to show, on a previous occasion,* that the emanations arising from decomposing matter do not, and cannot, in fact, give rise to Typhoid, or any other specific form of fever, it is unnecessary to dwell on this part of the subject now. The enunciation of the conclusion will be sufficient for my present purpose. By other writers, again, not only the spread, but the origin, also, of Typhoid fever is, in every case, referred to contagion. "The living human body," says Dr. Budd, "is the soil in which this specific poison breeds and multiplies; and that most specific of processes, which constitutes the fever itself, is the process by which the multiplication is effected."† As the fever exerts its influence, chiefly, on the intestinal canal, the contagionists conclude, that the disease is propagated more particularly from the emanations given out by the excreta, or by the introduction of this matter, and, with it, the contagious principle, into drinking water. But Dr. Murchison denies, that "the *fresh* stools passed by a typhoid patient are, in any way, deleterious." In proof of this, he states, that "during the last five years, cases of enteric fever, and cases which have not been fever at all, have been treated in the same wards of the London Fever Hospital: that 1,739 cases of the former, and 2,123 of the latter, have been interspersed together; and yet, that not one of the patients in these wards, has contracted enteric fever, although all the circumstances were most favourable to the propagation of the disease by the stools. Night chairs are placed between the beds, which are often indiscriminately used by the patients on each side: the pans are emptied only once a day, and no means are taken to disinfect the stools." And it is added: "The attendants, in these wards, have enjoyed a like immunity."* Other Practitioners have adduced similar facts. Still more improbable and hypothetical is the conclusion, that Typhoid fever is propagated by means of an infected drinking water: for all organic substances undergo decomposition, when introduced into the stomach—poisonous and putrid substances the same as others. Hence, the poison of the rattlesnake can be swallowed with perfect impunity; and has even been prescribed, as a remedy, by a Physician in America. Dogs also, in some experiments instituted at the Veterinary College, at Alfort, France, were fed on the diseased and purulent substances derived from horses, that had died of glanders and other diseases: but no ill effects were observed to ensue. Independently of these negative facts, no disease can be propagated from individual to individual unless an infectious matter be first produced: as such there must be a cause in operation, irrespective of contagion, otherwise it would be impossible to account for the origin of the disease, or of the first case.* If, therefore, there be a cause to which the origin of the disease is to be ascribed, not only in the first instance, but in the majority of outbreaks—for this disease has been springing up, during the last 50 years, in localities where it never prevailed before, and in all quarters of the globe,—it would appear to be more philosophical to refer the spread of the disease to the continued operation of the same cause, rather than to an additional one, or to contagion. All the facts, indeed, with which we are acquainted, are in direct opposition to this doctrine. Not only do those in attendance on the sick generally escape, but other members of the family also. In Paris, where fever patients are taken into the same ward with those suffering from other affections, no instance, as M. Andral has remarked, is known of the disease being communicated to those lying in the adjoining beds. Then, again, medical students, living, in general, in small confined rooms, attend their companions, attacked with Typhoid fever, and bestow upon them the most assiduous care; and yet, adds M. Andral, I do not remember a single instance of the disease having been contracted under these circumstances. What the real cause is of the production of Typhoid fever, it will be desirable, therefore, to ascertain.

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* The History, Diagnosis and Treatment of Typhoid and of Typhus fever. Philadelphia. 1842. Pp. 266 and 291.

† On the Identity or Non-Identity of Typhoid and Typhus fevers.

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* Vide Epidemiology, Chap. 2. † Typhoid Fever: its Nature, etc., etc.

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* British Medical Journal, March 16, 1867.

* "Il est evident," says Laplace, "qu'une chose ne peut pas commencer d'être, sans une cause qui la produise. Cet axiome, connu sous le nom de principe de la raison suffisante, s'étend à tout."

If we study the history of this disease, it will be found that Typhoid fever has, in certain localities, replaced intermittent fever. It must therefore be a malarious disease: this occurrence—the conversion of intermittents into other forms of fever—being a common one in malarious districts. In general, this result is to be referred to surface drainage. That the production of Typhoid fever is to be referred to the same cause, the following examples, among others, will be sufficient to prove. Mr. Cass, surgeon, of Goole, states that; "Thirty-seven years ago (or in 1826), when he first went there, intermittent fevers were very frequent, but *not* often *severe* in form." About this time, "extensive drainage of the land in this district was effected," the result of which has been told by Dr. Whitley, who remarked:—"There has been much typhoid fever of late years, and much zymotic disease prevailed at the time of my visit."* As may be remembered, a severe outbreak of Typhoid fever occurred at Winterton, Lincolnshire, 1865-6. Let us hear, then, what the Government Inspector, Dr. Thorne, says on the subject:—"Ague was, up to thirty years ago, very prevalent in the district; but, since the neighbourhood has been properly *drained*, *no cases* have occurred."† Exactly so; they have been replaced by typhoid fever. In addition to the outbreak in the above year, Winterton suffered for several years previously, although to a less extent, from the prevalence of the same complaint; while there was another outbreak in 1867. A recent writer has referred the origin of typhoid fever to the same cause. M. Boudin remarks: "The drying (or drainage) of the soil, or its conversion into a lake,* while producing the disappearance or diminution of paludal diseases, appears to predispose the organization to a new pathological condition, according to the locality, and in which phthisis and *typhoid fever* play a prominent part."† These circumstances afford an explanation of another phenomenon, otherwise inexplicable, connected with the prevalence of typhoid fever. It is evident, from the admissions into the London Fever hospital, that there have been fewer cases in wet seasons than in dry, as was particularly remarked in the dry season of 1858. "The opposite effect of drought and rain have been well illustrated," observes Dr. Harley, "during the present summer (1865). Towards the end of July, there was a great want of rain, but from the 30th of this month, and throughout August, there was an unusual amount, large quantities having fallen almost every day. As shown by the admissions into the London Fever Hospital, enteric fever was very prevalent during the dry season; but, after a fortnight's heavy rain, its further progress received a sudden check, which continued until the effects of the succeeding drought became manifest."* †

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* Sixth Report of the Medical Officer of the Privy Council, p. 444 (1863). † Id. for 1867.

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* To understand the cause of this apparent anomaly, vide Causation and Prevention of Disease, *Law* 3, p. 78.

† Etude de Géologie Médicale, 1848.

In order to show the connection between the two diseases, in another point of view, it may be remarked, that Typhoid fever frequently assumes an intermittent form. The Commissioners appointed to investigate the circumstances connected with the epidemics, that prevailed in France from 1841 to 1846, remark: "But there is, more particularly, one symptom, that epidemic typhoid fevers have often presented; and on which the commission thinks it right to call your attention, for a moment. It is the intermittent or remittent character, more or less malignant, which manifested itself in a great number of instances. The Physicians, who have described these epidemics in their Reports, have constantly attributed it to marshy or paludal emanations."‡ Dr. Harley also states, that "the camp fever of the army of the Potomac," as he was informed by Dr. Charles Mayo, "was generally recognised as a typho-malarious fever, in which the symptoms of typhoid fever, diarrhœa, rose rash, etc.

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* Reynolds' System of Medicine, Art. Typhoid Fever.

† For an explanation of the variation observed in these instances, see Causation and Prevention of Disease, and the Laws regulating the extrication of Malaria from the surface.

‡ Mem. de l'Academ. de Médécine, T. xiv., p. 71.

were associated with those of intermittent fever."* Again: typhoid fever, like ague, generally appears, or, at least, prevails principally, in the autumn. Hence, it has been termed in New England, America, the autumnal, or fall, fever. In other instances, and more particularly in England, Typhoid fever would seem to have superseded Typhus. If, therefore, the latter disease be a product of malaria, there is an additional reason for inferring, that Typhoid fever is also produced by the same cause.† That the two diseases are merely modified forms of one and the same complaint, may be inferred from the following facts. In the first place, the petechial eruption of Typhus, and the rose-spots of Typhoid fever, may co-exist in the same patient. In the next place, in cases of relapse, Typhus may be succeeded by Typhoid, and *vice versâ;* thus showing, that the same cause is productive of both affections. Then, again, the true pathognomonic symptom of typhoid fever—ulceration of the intestines—is occasionally found to exist in undoubted cases of typhus. "In the maculated typhus of 1826 and 7, there were," says Dr. Stokes,

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* Reynolds' System of Medicine, Vol. I., p. 602.

† It would appear, that Typhoid fever, with some few exceptions, only attacks an individual once in his life-time. The cause of the phenomenon will be explained hereafter, under the head, smallpox. Suffice it to remark now, that this circumstance will not invalidate the conclusion before drawn, that this disease is a product of Malaria.

"extensive ulcerations in the lower third of the ileum." On the other hand, there were, in a large number of typhoid cases, "typhus affections of the bronchial system"—one of the pathognomonic symptoms of typhus. In addition to the preceding, these various effects are sometimes seen alternating in the same patient; a sure proof that both diseases are produced by one and the self-same cause. Hence Rokitanskey has designated typhoid fever as *typhus abdominalis*, and typhus as *pneumo-typhus*. But, in the pure forms of these diseases, the distinction between the two is sufficiently apparent, as will now be evident.

SYMPTOMS.—The majority of persons attacked with this disease are in the prime of life, from 18 to 30 or 40 years of age, rarely above the last-named period. Few destitute persons are attacked: on the contrary, it would appear to single out those who live well, and who take a greater rather than a lesser quantity of nourishment. The course of this disease, generally speaking, is slow and insidious, the patient being able to continue his usual avocations for some days: and only complaining of want of appetite, pain in the limbs, lassitude, thirst, irregular chills, and headache. These effects are generally followed by diarrhœa, with or without colic; tinnitus aurium; a bitter taste in the mouth; the tongue being furred, or covered with a brownish mucus—*langue pâteuse*—nausea; vomitings; restlessness; urgent thirst, and fever, increased during the night. This may be said to constitute the first stage of the disease: the next is characterised by a persistent diarrhœa, or, else, by constipation;* tympanitis; a rumbling, or gurgling, in the bowels; distension of the abdomen, with tenderness in the epigastric region. It is at this period, that one of the characteristic effects of typhoid fever is observed —the rose-coloured eruption, composed of small lenticular spots, which disappear after two or three days, to be replaced by others. They are seldom observed before the 5th day; more generally between the 8th and the 15th. With the development of the preceding phenomena, the febrile symptoms also increase; the pulse rises in frequency, but is seldom strong, and is generally compressible. Stupor, or somnolence, which increases gradually in intensity, particularly in the fatal cases, is usually present at this period. Of 57 cases that recovered, and in which the disease presented a severe type, eight had no marked symptoms of stupor. With the others, the average duration of this state was eight days. Delirium accompanies the stupor, in a great many cases; seldom precedes it, and more frequently follows it, at an interval of 2, 3, 6 or more days. (*Op. cit.*, vol. ii., p. 15.) It may appear as early as the 5th day; but more generally in the 2nd or 3rd week. Intestinal hæmorrhage, which frequently occurs in typhoid fever, is characteristic of this disease, being seldom observed in typhus. It varies in quantity, from one ounce to sixteen or more, as, also, in colour. When red, it comes, apparently, from the ulcerated surface of the intestines; when dark, from the hæmorrhoidal or other veins. If there has been no amelioration of the symptoms previously to this,—some writers say, before the 5th or 6th day—the above symptoms become more intense; the vital powers more and more prostrate; and effects similar to those observed in the last stage of other fevers supervene, to be followed, sooner or later, by the last effect of all—death.

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* According to M. Louis, of 40 fatal cases, only three were unattended by diarrhœa. With the others, the diarrhœa commenced on the 1st day of the attack with 22; from the 3rd to the 9th day, with 9; and from the 11th to the 14th, with 6.

Of 57 patients that recovered, and who had the disease in a severe form, diarrhœa was present in all. Of these, 24 had diarrhœa from the commencement of the disease; 5, on the 2nd day; 3, on the 3rd; and 4, on the 4th day, etc., etc. With two, it appeared on the 18th and 30th day of the attack. The diarrhœa was severe (from 8 to 12 stools in the 24 hours, for one or two weeks) with 14 patients: slight, with 22 (from 2 to 4 stools in the course of the day); and of an intermediate grade with the others. The average duration of the diarrhœa, with these cases, was 26 days.

Of 31 patients, who had the disease in a mild form, diarrhœa was present in all, excepting two. It appeared somewhat later than in the preceding cases, and its average duration was only 15 days. (*Op. cit.*, vol. 1, pp. 430-39, 2nd Edition.)

Such are the symptoms, and such the course of the disease, in the majority of cases, or in those which assume a severe form. But in others, there are some remarkable modifications. Thus, many patients only experience, for a certain time, some feverish symptoms; thirst; a loss of appetite, and slight debility: without stupor; without diarrhœa; without pain in the abdomen; and without any symptom that indicates, with certainty, the seat of the disease. It is only after five, six, or eight days, that abdominal pain or diarrhœa is observed; from which time the disease follows its usual course. With some persons, the fever, after commencing with some intensity, disappears; the characteristic symptoms are not observed, and the patient appears to be suffering from a simple gastric irritation. This is what M. Louis calls the *latent* form of Typhoid fever, in which the diagnostic is doubtful until the moment of death; or, until perforation of the intestine removes all doubt on the subject. This termination of the disease, viz., perforation, which is characteristic of Typhoid fever, may occur at any period, even during the convalescence of severe and protracted cases; as well as in cases, in which no marked abdominal lesion has been observed, during life. The symptoms of this fatal event are as follows. Sudden and excruciating pain in the abdomen, increased by pressure: chills, followed by clammy perspiration; nausea and vomiting; altered expression of the countenance, and alarm; tympanitic distension; difficult breathing and death, within 30 or 40 hours.

An analysis of the symptoms now detailed, leads to the conclusion, that Typhoid is a low, adynamic, form of fever, with a particular and well-marked lesion of the intestinal canal. This conclusion is confirmed by the result of pathological investigations, as we shall now find.

PATHOLOGY OF TYPHOID FEVER.*—Commencing with the œsophagus, ulcers are frequently found on the internal surface of this tube: when few in number, at the cardiac extremity only, but occupying its whole length, when more numerous. M. Louis considers the affection of the œsophagus to be peculiar to this disease, not having observed the same state in other acute diseases. In two instances, mentioned by Dr. Barth, there was perforation of the œsophagus. In the stomach, the mucous membrane was found more or less profoundly altered in the majority of cases: it was softened, and diminished in thickness, in a 6th of the cases examined by M. Louis, and in a greater proportion by M. Chomel.† Mamellonation, consisting of small elevations of the mucous membrane, either scattered over the whole surface of the stomach, or, else, confined to the pyloric extremity, existed in two-sevenths of the cases. Ulcerations of this organ, as distinguished from destruction of the mucous membrane, by the exact limitation, clean, sharp edge, and small size, of the ulcers, are only occasionally met with in this disease. These pathological conditions of the stomach, however, are not peculiar to typhoid fever: they are also observed in other and acute forms of fever, although to a less extent. On the other hand, the mucous membrane of the stomach was found to be in its normal condition with two-sevenths of the subjects, that had died of typhoid fever, and with only one-fifth of those carried off by other acute diseases. (*Op. cit.*, vol. i., p. 156.) As regards the duodenum, it was found to be in its normal condition in the majority of cases; its mucous membrane softened, and covered with rose-coloured or red spots, in a few cases, and with small, superficial, ulcerations near the pylorus in still fewer—in two subjects only. Passing onwards, it is in the ileum that we shall find the true anatomical lesion of Typhoid fever. In a certain number of cases, or about a fourth, the mucous membrane preserved its normal colour—white or pink-white; in others, there were patches of a red tint, and, in some, those in which the disease had existed for a longer time, the patches were of a red or ash grey. The membrane was also found to be softened in about a fifth of the cases, or in nine out of forty-three.

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* M. Louis has given the histories of 50 fatal cases of Typhoid fever, but the pathological results observed were derived from 46 cases only. Of these six died between the 8th and the 15th day of the disease; seven, from the 16th to the 20th day; twenty, from the 20th to the 30th day, and nine after this period.

† Leçons de Clinique Médicale.

The chief anatomical character of Typhoid fever consists in the changes which the agminated glands, or the patches of Peyer, undergo. These changes vary according to the intensity of the disease, and its duration; they are also greater towards the ileo-cæcal valve, or in the lower rather than in the upper portion of the bowel. The earliest change would appear to consist in a swelling of the mucous membrane of this portion of the bowel, with slight elevation of the affected glands. This is followed by a thickening of the glands, caused by the deposit of a white substance in the sub-mucous tissue; the patches varying in size from an $\frac{1}{8}$ or a $\frac{1}{4}$ of an inch to an inch and a half; and, occasionally, to several inches. In some instances, the patches are redder, larger, and softer, as, also, the sub-mucous cellular tissue; while in others, the patches are hard—*plaques dures.* This last state was observed principally in cases that ran a rapid course, and when death occurred from the 8th to the 15th day. The next change that the patches of Peyer undergo is that of ulceration. A slough forms in the tumified, or infiltrated gland, which is separated and thrown off, after a time, leaving an ulcer on the internal surface of the intestine. The size of the ulcers varies from that of a hemp-seed or a pea to that of a half-crown; they are principally found in the lower third of the ileum, and increase in number as they approach the cæcal valve. The solitary glands are also affected, but the ulcerative process, in these, takes place at a later period, and is less rapid than in the agminated glands, or patches. In other cases, instead of an ulcer, a fungus growth is produced in the diseased gland; an alteration that is the frequent cause of hæmorrhage.

Not only may the deposit in the glands undergo resolution, but the typhous ulcers, as they are termed, may cicatrize and heal. Hence the ulcers, in some cases, are found in different states after death: some healed, others in process of cicatrization: while, in the remainder, the ulcerative process has destroyed the mucous and muscular coats—the floor of the ulcer resting on the peritoneum only. In other instances, again, the peritoneum has been perforated, thus allowing the contents of the bowel to escape into the peritoneal cavity. In general, the perforation is small, not larger than a pin's head; and is situated in the centre of the ulcer. When sloughing takes place, however, the aperture is much larger: there may also be several apertures, but this is a rare occurrence, one perforation being sufficient to produce death. As to the proportion of cases of perforation, M. Louis sets it down at 15 per cent.: this, at least, was the proportion in his own practice.

The glands in the large intestines are also affected, in some cases, and ulcerated; but the ulcers are small, few in number, and generally confined to the cæcum. The mesenteric glands are almost invariably diseased, more especially those near the glands of Peyer: when these are ulcerated, pus is generally found in the former. As the pathological condition of the small intestines, or, rather, of the glands of Peyer, exists constantly in Typhoid fever, and has not been found, with some few exceptions, in any other form of fever, it must be regarded as the anatomical characteristic of this affection. We may conclude, in fact, as M. Louis remarks, " that every acute disease, which is not accompanied by a specific alteration of the glands (*plaques*) of Peyer, is *not* a typhoid affection, even when we have been unable to find, by examination after death, the marks of another disease." (vol. ii., p. 317).

It is unnecessary to refer to the other morbid appearances, as they are common to the majority of fevers. There is, however, one lesion which is peculiar to typhoid fever, and ought not, therefore, to be passed over. This is an affection of the spleen; which is found enlarged, and altered in consistence and in colour. The organ was 3, 4 and 5 times larger than in the normal state with 17 out of 46 patients, whose histories were collected. In the other cases, if we except 10, the spleen was twice or more than twice the natural size. With the above 10, the organ was either of the normal size or less than double the size. "This increase," adds M. Louis, "of volume of the spleen, during the progress of typhoid fever, is a fact respecting which all observers are agreed; and to mention one instance, I will only add that, of 32 patients carried off by typhoid fever, the histories of which have been carefully collected by M. Barth, only one had a small or normal spleen." Again: a softening of the spleen, to a greater or less extent, was observed in three-fourths of the cases, or with 31 patients—slightly more than the number with an excessive enlargement of the organ. With 7, the softening was great; to an inferior degree with 7 others, and to a less degree with the remainder. The excess, in the size and softening of the spleen, was observed principally with those who were carried off at an earlier period of the attack. "If then we remember," observes M. Louis, "that the spleen was not quite healthy, that is to say, both of a natural size and consistence, in any case in which death took place before the 18th or 20th day of the disease; that it was not exempt from all alteration, excepting with four subjects, who died at an early period of the disease: we may conclude, that the alteration of this organ commences at an early period of the attack, and *very probably* it takes place in every case." (vol. i., pp. 260—62). The colour of the spleen was no less changed than the size and consistence, presenting, however, a great variation, according to the intensity, and the duration of the attack. "Hence," concludes M. Louis, "the more we analyze, the more we consider, these facts, in different points of view, the more we shall be convinced, that the alteration of the spleen has a something special and characteristic in it, with the subjects attacked with typhoid fever." These results are confirmative of the conclusion before drawn, viz., that typhoid fever is merely a modified form of intermittent fever; for if there be one lesion, more than another, characteristic of the latter disease, it is the enlargement and disease of the spleen. With this summary of the symptoms and pathology of typhoid fever, we shall be better enabled to understand the rationale of the treatment about to be proposed.

TREATMENT.—If the deduction now drawn be allowed, viz., that typhoid fever, like Typhus, is produced by the same cause as intermittent fever, it follows, as a matter of course, that the treatment which is beneficial in the one disease will be equally applicable in the other. The beneficial result, however, may be very different, intermittent fever and typhus being simple, uncomplicated, forms of fever; typhoid fever one of the most complicated, and attended with a particular and dangerous lesion of the intestinal canal. Let us inquire, then, what difference this circumstance will make in the results obtained by the employment of the same remedy, or antidote. It might be concluded, on a superficial consideration of the question, that this is precisely the case in which the administration of such an agent would be the most beneficial, for the antidote would be brought into immediate contact with the diseased intestinal surface. But, although such is the fact, it does not follow, that the antidote should be brought into contact with the poison productive of the morbid effects. These are all due to inflammation, which is an affection of the capillaries; produced, according to the deductions previously drawn, by the presence of the morbid agent in these vessels. As we have no reason to believe, that the capillaries perform the function of absorption, the antidote, in all probability, would have to perform the circuit of the circulation before reaching the morbid matter. But, as a great part of the remedy, or the carbonic acid gas, escapes out of the system by the lungs, there would be the same difficulty in neutralising the poison, contained in the intestinal capillaries, as is experienced when the same matter is confined in the capillaries of the skin. Still, as the administration of the antidote by the mouth is generally sufficient to remove the morbid effects in the external capillaries, in slight cases of fever; we may conclude, that the intestinal inflammation, in Typhoid fever, will also be removed by the same means, under the same circumstances. This, in fact, is the result that has been obtained by me in the first stage of this disease; but whether it has been obtained directly or indirectly is somewhat difficult to say. The probability is, that it is produced indirectly. As the intestinal inflammation is not a primary affection, but merely an effect, among many others, of an antecedent cause; and as this cause, according to the arguments previously advanced, is the presence of a poison in the blood, it follows, if the greater part of this poison be neutralized, that the effects would either cease, or, else, be arrested in their further course. Such we have a right to infer would be the case, by the administration of an antidote, if given at an early period of the attack; and particularly in an adynamic fever like this, when the greater portion of the poison is contained in the venous system. Hence, as I should infer, the benefit that is derived, from the employment of carbonic acid gas, in the first stage of Typhoid fever: for not only have the diarrhœa and the intestinal affection been removed, but the fever also and other morbid effects. It may be that some effect is produced directly as well as indirectly. As the inflammation, like the fever, is of an asthenic, not a sthenic, character, with a state of congestion rather than of increased circulation, a portion of the gas, after its introduction into the stomach, may be absorbed by the venous radicles of the part affected, and thus find its way directly into the capillary vessels. But whatever explanation may be offered on the subject, one thing is certain; the course of the disease, according to my experience, can always be arrested by the administration of carbonic acid gas at an early period of the attack. When, however, from the continuance of the inflammation, or from its intensity, disorganisation and ulceration of the mucous membrane have taken place, the employment of an antidote can no more remedy these effects than the administration of an alkali can remove the ulceration and gangrene, that may have been produced in the stomach by a poisonous dose of sulphuric acid. The following case will prove this. When in Rome, some years since, I was asked to visit a Lady, attacked with Typhoid fever, in consultation with Dr. Deakin. She had been ill ten or twelve days, and, at the time of my visit, had high fever, with all the symptoms of inflammation of the bowels—to relieve which large cataplasms had been applied to the abdomen, in addition to the ordinary internal remedies. With the concurrence of Dr. Deakin, who was acquainted with my previous treatment of intermittent fever, by the same Agent, an effervescing draught was ordered to be taken every two hours; and all other remedies to be discontinued. On my second visit, in the evening, the fever had greatly subsided, and had nearly or entirely vanished on the following morning: so relieved, in fact, did the patient feel, that she thought it unnecessary for me to repeat my visit. In the evening, however, I was again summoned, and found the patient in a state of partial collapse, with intense pain and distension of the abdomen; while the look of contentment and pleasure, expressed in the morning, had been exchanged for one of terror and alarm. From this state she never rallied, having expired early the following day. No *post mortem* examination was made, but the cause of death was sufficiently apparent. There must have been ulceration or gangrene of the intestines with perforation.*

Independently of the intestinal inflammation, it may be impossible to remove the fever, although it is generally of an adynamic form, when the antidote is administered at a late period of the attack. This will be the case, if absorption be suspended in this disease after a certain interval, the same as in other fevers: which would appear to be the fact. "In certain cases of Typhoid fever, absorption," says M. Bernard, "remains suspended for a considerable time. This has been shown by administering to the patient small quantities of prussiate of potash, no trace of which can be discovered afterwards in the urine or other secretions." †

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* I am inclined to believe, that there was gangrene, in this case, rather than ulceration, and for the following reasons. This Lady, who suffered from Menorrhagia, had been in the habit of taking strong astringents; remedies that not only kept the bowels confined, but which also tended to arrest all the intestinal secretions. To this circumstance I attribute the more acute form of the inflammation and fever; as, also, the speedy and fatal termination. It may, perhaps, be considered, that this was not a typical case; as, however, the disease was prevailing generally in Rome, at the time, there can be little doubt on the subject.

† Leçons de Pathologie Expérimentale, p. 28.

Considering how important it is to remove the intestinal inflammation, and how problematical it is, that the poison contained in the capillary vessels can be neutralized by the employment of the gas alone, it would seem desirable to administer other forms of carbon—those which become component parts of the blood, and, being carried forward into the extreme divisions and subdivisions of the arterial system, are there converted from the solid into the gaseous form. Such are the hydro-carbons, the therapeutical effect of which has been previously considered. The best hydro-carbon is oil, but it is difficult of digestion; while its administration is contra-indicated, as long as there is any nausea or vomiting. The only plan, under such circumstances, would be to trust to ripe fruits, oranges, and lime juice, combined with a large proportion of sugar; or, the latter alone, if the acid appears to disagree with the stomach or bowels. Farinaceous substances should also be given, and particularly those that contain an excess of carbon—such as sago, rice, tapioca, gruel, and arrowroot, although the former, considering the state of the stomach, are more likely to be digested.

As regards the employment of other and ordinary adjuvants, it will be unnecessary to give any directions. These will necessarily depend on the opinions and the judgment of the medical attendant. I would merely observe, that those remedies should be employed which are the most likely to expel the poison out of the system; not those which tend to shut it up hermetically within the containing vessels. This result would inevitably occur by the employment, in order to check the diarrhœa, of opium and strong astringents; such as the mineral acids, diacetate, or sugar, of lead, sulphate of copper, nitrate of silver, alum, and the vegetable astringents —krameria, logwood, and catechu—"which," remarks Dr. Tweedie, "like the mineral astringents, are most efficacious when combined with opium." "These agents," adds the writer, "when judiciously managed, will do all that an individual remedy can effect in controlling the diarrhœa." (*Op. cit.*, p. 234.) But the diarrhœa ought not, in my opinion, to be controlled by such remedies; for this effect is merely an effort of nature to expel the poison out of the system, and should be encouraged rather than suppressed. In proof of this inference, it is only necessary to refer to the facts previously adduced, viz., that of 40 fatal cases, three were unattended by diarrhœa; while of 57 severe cases that recovered, diarrhœa was present in *all*. It is not surprising, that there should be perforations of the intestines, when remedies like these are had recourse to; the effect of their employment being to arrest the abdominal secretions, and to increase the inflammation of the mucous membrane. Not only are such agents injurious, but they will be unnecessary, if a proper mode of treatment be adopted. Remove the cause and the effect will cease; and this I have invariably found to be the case by the employment of carbonic acid gas, when administered at an early stage of the disease, or before absorption becomes suspended. In the absence of an antidote, those remedies should be employed, which increase the abdominal and other secretions, not those which suppress them.

Not less injurious are narcotics, and remedies that depress the vital energies, such as digitalis. This dangerous Agent has been employed on the Continent, of late years, in acute specific diseases and fevers, in order to reduce the temperature and the frequency of the pulse. According to Wunderlich, it is especially efficacious in the 2nd or beginning of the 3rd week of Typhoid fever, when the temperature is high, and the pulse rapid. But this form of the disease is not the ordinary one; while it is less dangerous than the asthenic form. The reason is evident. In the former case, the poison will be principally contained in the capillaries of the skin; in the latter, in the internal and vital organs. When, therefore, a drug, which is neither an antidote nor a diaphoretic, and which acts merely by depressing the nervous energy, and retarding the action of the heart, or the general circulation, is employed, what is likely to follow? Instead of expelling the poison out of the system, it will be thrown back upon the internal and vital organs, and be productive of injurious or dangerous results. That this conclusion is not hypothetical, will be evident from a perusal of the following case, in which this drug was the principal agent employed.

It is that of a little girl, æt. 10, admitted into University College hospital, January 2nd, 1874, under the care of Dr. Ringer. The symptoms, when first seen, were as follows. Slight headache, pain in the right iliac region, with only slight tenderness, no lung or heart complication, but slight enlargement of the spleen. No rash on the body or limbs; skin moist, tongue moist, but furred, urine high coloured, and free from albumen. The pulse quick and compressible, 130, the temperature of the rectum 104. On the 4th (8th day of the fever), the patient commenced taking 1 gr. of digitalis every two hours—taking generally 8 grs. in the 24 hours. In the evening the pulse was reduced to 116, and there was no exacerbation of the fever. On the following evening (5th), there was a further fall of 16 beats in the pulse, and a slight reduction of temperature. On the 8th, the patient threw up the powder, but an equivalent dose of the tincture was substituted. On the 9th, a few rose-coloured spots appeared on the body, the pulse was reduced to 66, sharp, but not compressible, and irregular—the irregularity consisting of 3 or 4 semi-pulsations between every 3rd or 4th beat. At 11 p.m. the pulse was 64, thus showing a reduction of 68 beats in 6 days, and of 44 beats in 24 hours; the respiration had also been reduced from 32 to 20. On the 11th, the patient was ordered brandy (2 oz.), the digitalis to be continued as before. There was a further eruption of typhoid spots. On the 14th, there was an elevation of temperature to 104·4, the pulse being 72, and slightly irregular. In consequence of the rise of temperature, an addition of 5 m. of the digitalis was ordered. On the 15th, the pulse at 5 different periods of the day was 70, 106, 80, 60, and 90—fairly strong, irregular, but not intermittent. After a restless night, attention was drawn to the patient by a gurgling sound, the pupils were observed to be dilated, the pulse scarcely perceptible, and in 2 or 3 minutes the little girl was a corpse. On examination of the body, small ulcers—varying from a pin's head to a split pea—

were discovered in the lower third of the small intestines, but there was no perforation.*

If such was the result, in a comparatively mild and uncomplicated case like this, what is to be expected, when the vital powers are in a state of great depression, as is so generally the case? We can imagine what the result will be from a circumstance mentioned by Dr. Ringer. He states that, in two cases of *delirium tremens*, in which digitalis was administered, "the patient suddenly fell back dead, although, to the moment of death, there had been nothing to indicate the probability of this sudden and untoward event." With these results before us, it is to be hoped, that Typhoid fever will hereafter be treated scientifically, not empirically, even if we should be unable to arrest its progress, and lessen its mortality, by the employment of a more specific and certain remedy.

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* Abridged from the account of Dr. W. Murrell, in the *Practitioner*, November, 1875, p. 345.

CHAPTER VII.

THE TREATMENT OF PUERPERAL FEVER.

UNDER this term, several forms of fever, not only distinct in character, but evidently arising from different causes, are usually comprehended. Busch enumerates the following varieties of puerperal fever: (*a*) Gastric fever, (*b*) Nervous fever, (*c*) Typhus fever, (*d*) Petechial fever. Dr. John Clarke divided this fever into three forms. 1st. That connected with, or arising from, local inflammation. 2nd. Primary, synochal fever, developing local inflammation; and 3rd, Typhoid (typhus?). fever. The classification of Dr. Ferguson embraces, 1st. The *inflammatory* form. 2nd. The *Synochoid*, and 3rd, the *Adynamic*. That these various affections should be observed is not surprising, lying-in women being exposed to the operation of causes from which persons in health are exempt. During gestation, certain of the organic functions are interfered with, and are frequently deranged. So also in labour, when it is protracted, difficult, or unusually painful, the system frequently receives a shock, particularly in nervous and delicate females, from which it cannot immediately rally. Hence the inflammatory and nervous forms of fever which are so frequently observed at this particular period. But this class of persons are not only liable to individual, morbid causes; they are equally exposed, with other classes, to the operation of external agents; and hence the typhus, typhoid, petechial, and synochoid, forms of puerperal fever.*

It sometimes happens, however, that puerperal fever prevails epidemically, when no similar disease, as typhus, is observed among other classes; and it is then generally referred to infection, all sorts of idle tales being told, in order to account for its spread—such as the infection being carried from patient to patient by the Medical Attendant, as though Doctors never washed their hands, or changed their clothes; and were carried direct, by electric telegraph, from one patient to another! In 1818 and -19, puerperal fever was so prevalent in Paris, and the environs, that 300 women died of it, in the *Maternité* of that city alone, in the course of these two years. But the visiting Physicians, MM. Tonnelé and Dugès, state, that the disease was not propagated by contagion; one reason, among others, being that when a patient, suffering from fever, was admitted, those in the adjoining beds did not take the disease. This conclusion is in accordance with the opinion of some of the most experienced Accoucheurs. Dr. Hulme states, that puerperal fever is not an infectious disease:* and Home says, that a patient in the Royal Infirmary, Edinburgh, who was delivered on the *same bed*, on which a puerperal patient had died a few days before, and on which she continued to sleep until her convalescence, did not take the disease.† Burns also, with his great experience, observes, that he was never able to trace the infection from one patient to another.‡ A more recent writer, Dr. Campbell, says: "There were no grounds for considering the Puerperal fever, as far as fell under my observation, to have been of an infectious nature."§ In proof of this, the writer states, that several cases of puerperal fever occurred, although neither the patient, the doctor, or the nurse had been in contact, previously, with any person labouring under the disease. Although it may appear singular, that this fever should be confined to a particular class; and that it should prevail, when no similar affection is observed among other classes, the phenomenon is not peculiar to this disease.

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* Dr. Collins states, that puerperal fever has been observed to become epidemic in the Dublin Lying-in Hospital, on several occasions, when typhus fever prevailed in that city; and at other periods, when erysipelas was frequent.

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* A Treatise on the Puerperal Fever, p. 164. 1772.

† Clinical Experiments p. 81.

‡ The Principles of Midwifery.

§ On the Epidemic Puerperal Fever in Edinburgh, in 1821-22, p. 216.

It is in fact a law, almost constantly observed, for epidemics to attack by classes. Thus, at one visitation of the plague, the old would be principally attacked; at the next, the young; and, at another, the middle aged and the strong. At one period, pregnant women were nearly all carried off; at another, they as generally escaped. But the most singular circumstance is, a large number of the students, at the German Universities, being attacked with plague, almost simultaneously, during the *vacations*, and while residing at home; although separated hundreds of miles, perhaps, from each other; and although the disease was not prevailing in the place or places of their abode at the time. The same phenomenon was observed with the "sweating sickness," or *sudor anglicanus*, which was confined to the English during its first invasions; not only the inhabitants of these islands being attacked, but Englishmen residing abroad, in the midst of a population that remained entirely exempt. There can be no occasion, therefore, to look to infection, when puerperal fever is prevailing epidemically in a town, or in an institution, or hospital; as we can account for its prevalence without reference to this imaginary agent. In addition to the preceding, "an epidemic year, or any year in which the malady is fatal here, is," says Dr. Ferguson, "also fatal in most parts of Europe; only the intensity of mortality in any part of that year varies in various countries, according as their seasons are earlier or later, and according to other partial causes."* The disease, therefore, must be produced by general, or external, not internal causes. Hence various writers —as Hulme, Leake, Home, Gardien, and several continental authors, as Capuron, Bang, Emerius, etc. —have ascribed the disease to atmospherical causes, or variations of temperature; some, to a noxious principle in the atmosphere. "Whenever," says Leake, "it is remarkably frequent and fatal, at particular seasons, its proximate cause ought to be referred to a *noxious constitution of the air*."† Besides, if this form of puerperal fever—the typhus or typhoid—be identical with ordinary typhus; and if the arguments previously employed be of any value, we must infer, that it is a product of Malaria; if so, it cannot be a contagious disease, for no one, in the least acquainted with the properties of this agent, would conclude, that a malarious fever is, or can be, propagated by contagion. That this form of puerperal fever is, like its congener—typhus—a malarious disease, may be inferred from other circumstances besides the preceding. In the 1st place, it frequently assumes a remittent or intermittent form, a sure sign of the presence of malaria in the system. An epidemic, that prevailed in 1775, was described by Dr. Butler under the name of *remittent* puerperal fever. Dr. Ferguson also states, that the symptoms very often assume those of remittent, and, occasionally, of intermittent fever. Dr. Campbell says; "in every case but three, that fell under my observation, the disease was ushered in by a distinct rigor, or a succession of rigors, which differed both in point of severity and duration. . . . . To the cold stage succeeds, in most cases, an intensely hot, parched skin. . . . . This intense heat is, sooner or later, followed by profuse but partial sweats. The perspiration is chiefly confined to the trunk of the body, and, if we have not been called until after it has subsided, we shall then find the temperature of the body very little beyond the natural standard." Hulme also remarks; "The skin is sometimes so cool and temperate (during the remission), that a person from thence could hardly know, whether the patient laboured under any disease or not." (*Op. cit.*, pp. 27, 28.) Another circumstance indicative of the influence of Malaria, is the fact, that puerperal fever prevails principally at a particular period of the year. In Paris, the greatest number of deaths, from this disease, are in the months of October, November, and February; in Geneva, in November, January, and March; in London, in December, January, February, March, and May; in Edinburgh, in November, December, and January; and in Aberdeen, in October, November, and December. The most favourable months are June in Paris and in Geneva; July in London;* and August in Scotland. These results are not only irreconcilable with the doctrine of contagion—for an infectious disease ought to spread more readily in hot than in cold weather—but they coincide with those observed with the majority of malarious diseases, as typhus fever, which prevails principally in the winter and spring, and Typhoid fever in the autumn.

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* Essays on the Most Important Diseases of Women, p. 102. London, 1839.

† Practical Observations on the Child-Bed Fever, p. 100. 1775.

There is, however, another form of puerperal fever, which assimilates to typhus, and which is produced by a different cause. By way of distinction, it may be termed the *adynamic*. It evidently arises from individual, or internal, causes; as the suppression of the lochia, the retention of the placenta, or a part of it, and its putrefaction; and, lastly, the death of the fœtus, and its partial decomposition before its expulsion. Dr. Campbell says: "Fever, arising from retention and putrefaction of the secundines, is identically the same with the noted puerperal fever; for it is accompanied with the same symptoms—intolerable headache from the commencement; prostration of strength; soreness of the abdomen; fœtid breath, and, ultimately, diarrhœa, with black vomiting." (*Op. cit.*, p. 204.) And Kirkland adds: "It sometimes happens, that coagulated blood lodges in the uterus after delivery, and putrefying from access of air, forms a most active poison; is in part absorbed, and brings on a putrid fever." (Id.) As, when putrid substances are absorbed into the blood, they produce effects very similar to those caused by the operation of malaria in the system, we can readily understand, why a purely adynamic, or putrid, form of fever should supervene under these circumstances.

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* Dr. Ferguson states, that no death had occurred from puerperal fever, in the General Lying-in Hospital, London, in the month of July, during the 12 previous years—1826-38. (*Op. cit*, p. 101.)

As regards the symptoms of puerperal Typhus, it will be unnecessary to describe them here, as they are nearly identical with those observed in Typhus, and other adynamic forms of fever. It is only necessary to remark, that the mulberry rash of Typhus is rarely, if ever observed; but the general surface assumes a lurid, dusky hue, while livid spots, or streaks, (*vibices*) are generally present. It may also be mentioned, that typhus, like small-pox, scarlatina, and measles, when it attacks a lying-in woman, generally assumes a more severe and malignant form than with other persons.

TREATMENT.—It would be superfluous, after the remarks previously made respecting other forms of fever, to enumerate the various remedies that have been employed in this disease. It is sufficient to know, that they have invariably been less beneficial than with ordinary typhus. The fatality of this disease has, in fact, commanded the attention of Practitioners from the most remote period to the present day. Hippocrates regarded it as not only dangerous but mortal. He observes, *morbus hic lethalis est, et paucæ effugere possunt.* Dr. Ferguson states, that " of 26 patients admitted into the General Lying-in Hospital, in 1838, 20 died—being a mortality of 76 per cent. The malady commenced in January, in which month Dr. Rigby saved only *one out of nine.* The hospital was closed for a month, and opened again in March, when he succeeded in rescuing only two in eight. Thinking that another mode of treatment might be more successful, I determined to bleed largely and to salivate . . . but three only in nine lived. Seeing that *no mode of treatment* was of avail, the hospital was closed from May to November." (*Op. cit.*, p. 277.) Such being the results, the principal subject for consideration now is, can a more successful method of treatment be proposed or adopted?

If the deductions previously drawn be allowed, there can be no doubt as to the mode of treatment that ought to be adopted for the typhus, or typhoid, and adynamic forms of puerperal fever—and I am not now treating of any other form. The treatment should be the antiseptic, or antidotal. This method has, in fact, been already adopted and proposed, long since, by two different Practitioners. Effervescing draughts were given by Hulme in puerperal fever, not alone, but in combination with other remedies. He remarks:—"In the intermediate spaces of time, the physician may interpose the saline draughts of Riverius. These draughts, in order to be of any service, should be repeated often, and may be given, either in the act of effervescence or otherwise, as the prescriber shall think proper. They may answer several purposes: they may operate as antiseptics, and assist in destroying the putrescence lodged in the bowels: they may also provoke urine, and help to quench thirst; and they have this further advantage, that they neither bind nor heat the body."* (*Op. cit.*, p. 61.) Dr. Leake also observes:—"Where the stools are excessively putrid, it might be worth while to try the effect of fluids, which contain a large quantity of *fixed air*, given in clysters; as they have been found powerfully to resist putrefaction, agreeable to some late hints in Dr. Priestley's curious tract on the method of impregnating water with fixed air." Dr. Leake also recommends the gas to be given by the mouth, for he says, in another place; "Where the signs of *putrefaction*, or an intermission of the fever appears, *antiseptics* and the Peruvian bark may be administered." (*Op. cit.*, p. 113.) Not that the writer had any faith in the bark, for he adds:—"It were to be wished, that the efficacy of that sovereign remedy, the Peruvian bark, might secure the patient from danger: but both reason and experience, as well as the very nature of the disease itself, in a manner excludes such hopes of relief. . . . . Sometimes, the bark increased the purging: and even brought it on again after it had ceased." (Pp. 136 and 137.) Whether these suggestions were followed, I am unable to say; but no cases illustrative of this mode of treatment have been published, as far as I am aware. Nor have I had an opportunity of treating such a case myself. But my friend, Mr. Alfred Moore, who was acquainted with my opinions, as to the value of carbonic acid gas in fever, administered this remedy in one particular case; although the notes of it, which he kindly sent to me, at the time, have been lost in common with all my papers; and Mr. Moore has not, I find, kept a copy himself. He has, however, been good enough to furnish me with the following particulars from memory:—

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* His prescription was as follows:—Sal Absinthii ad dr. j.; in succi limonum recentis cochleari exhibitum.

2, Bessborough Street, Pimlico,
October 26th, 1877.

MY DEAR SIR,

A case of Puerperal fever, from its anxiety, always remains impressed on the mind of the busy Practitioner. The case, the notes of which I sent you at the time, occurred about six years since. The labour was somewhat difficult; and being attended with hour-glass contraction, the placenta had to be removed. Three days afterwards, to my horror, symptoms of Peritonitis made their appearance; the abdomen was enlarged, with tenderness on pressure; the tongue black and furred; the pulse 120, with thirst, hot skin, and head-ache, &c. As black streaks (*vibices*) appeared over the abdomen, together with other symptoms indicative of a putrid, or adynamic, form of fever; and there being no amendment, I ordered, as a *dernier ressort*, effervescing draughts, composed of the bi-carbonate of potash and citric acid. Although approaching the house with some trepidation, so great was my anxiety, I fancied, on my visit the next day, that the abdominal streaks were not quite so black: the tongue also was less furred; and there was a slight amendment in the general symptoms. On the following day, the streaks had assumed a red colour; the pulse was reduced to 100; and the general state of the patient had greatly improved. From this time, the improvement, to my great surprise, was regular and rapid: and, on the 5th day of the attack, a piece of *putrid* placenta came away; to the retention of which the morbid effects may doubtless be ascribed.

I remain,
Yours sincerely,
Alfred Wm. Moore.

John Parkin, Esq., M.D.

Although this is the only case, that I am enabled to adduce, of the employment of carbonic acid gas in puerperal fever—and that, too, in the adynamic form only—the result, it is to be hoped, will be sufficient to induce other Practitioners to give the remedy a further trial, not only in this particular form of the disease—the adynamic—but in others also, and particularly in puerperal typhus.

CHAPTER VIII.

TREATMENT OF THE EXANTHEMATA.

SMALL Pox.—It is so universally believed, that this disease is not only propagated by contagion, but that it originates in this way, and in this way only, that it will, no doubt, be considered an act of downright heresy to doubt the truth of the conclusion. But belief alone, however general, and however firmly rooted, without demonstrative proof, can never establish the truth of any doctrine or any theory. For instance, it was universally concluded, until Harvey's immortal discovery, that the arteries contained air: but this belief did not prevent the blood from circulating in these vessels. Nor will the opinion, that small-pox is produced and propagated by contagion alone, prevent the operation of other agents, if they be concerned in its production. Now there are certain anomalies, as regards the propagation of this disease, that the Contagionists have never explained, or even attempted to explain.

In the first place, we have never been told how this infectious matter was produced at first; for this disease was unknown in Europe and in Africa, until the middle of the 6th century: nor has it yet been decided, whether the infectious matter has been preserved intact during all this period, or whether it be re-produced at each visitation. Sir Thomas Watson concludes, that chicken-pox, small-pox, typhus fever, typhoid, scarlet fever, the plague, measles, hooping cough, &c., have no other origin than contagion, and never spring up, in our time, *de novo*. To suppose, however, that a specific matter could be preserved for so many centuries, and in all quarters of the globe—in the torrid zone as well as in the Arctic regions—and during the intervals of its attacks, is so preposterous an idea, that it would hardly appear to be worthy of a serious refutation. If, however, it be not shut up in Pandora's box, during these intervals, what are the circumstances, or combination of circumstances, we may ask, by which this matter is formed, *de novo*, at each visitation? Is it likely, that the same accidents, and the same combination of circumstances, should be found on the deserts of Arabia, on the alluvial plains of India, on the frozen steppes of Russia, in the temperate climate of England, and in the mountainous regions of Mexico? Such a combination of circumstances, under such varied contingencies, would appear to be so improbable, that I, for one, shall never believe in the production of a specific poison in such a way as this—an agent, be it remembered, that has not varied either in its composition or in its effects, any more than prussic acid has done, during all these ages, and in all these different countries. Still less likely is it, that a cause should exist, by which the morbid matter is produced and re-produced in the bodies of men, and with so many different races—the Englishman and the Hindoo, the Arab and the Russian, the Negro and the Greenlander—whose habits, modes of living, and food, are so diametrically opposed to each other. Besides, the visitations of small-pox are not accidental: the returns of the disease, during the last pestilential epoch, having been very regular, at least in the same country—about every seven years in India, every ten in England, and every 17 or 18 in Mexico; it must therefore be governed by immutable laws, like those that govern the universe itself. As such, it would seem to be a more profitable occupation to study these laws; and so proceed from effects to causes, rather than to indulge in idle dreams and vain speculations, which can only be regarded as cloaks for ignorance, and the offspring of egotism—too proud to say, *I do not know.*

Scarcely less difficult is it to account for the propagation of small-pox, from individual to individual, by the doctrine of contagion; it being impossible, in the majority of cases, to trace the source whence the infection was derived. The late Dr. Gregory declared, that "of the numerous cases received into the Small-pox hospital, not one in 20 was capable of being referred to any known source of infection." On the other hand, there are many facts which lead to the conclusion, that the disease is not contagious. In the first place, if this disease were propagated by contagion, a small-pox hospital would be a mere charnel-house: the emanations from 20 or 30 patients, congregated in one ward, would become so concentrated, that not a single inmate could escape with life. And yet, the rate of mortality is not greater in these institutions, all circumstances taken into consideration, than what it is in private practice. In the next place, the medical attendants and nurses, in these hospitals, are not more liable to be attacked than other persons; a sure proof that the disease is not contagious. A reason, it is true, has been assigned for this immunity, viz., that these persons have been all vaccinated, if not re-vaccinated. But vaccination will never prevent attacks of small-pox: the proof of which is, that four-fifths of the patients admitted into the hospitals, during the late visitations, had been previously vaccinated. Cases of small-pox, after vaccination, were rare at the commencement of the present century; but, in 1819, there were 19 cases admitted into the Small-pox hospital, London. In 1825, 147 were admitted, since which time these cases have been gradually increasing. From 1835 to 1851, the proportion of *vaccinated* cases was 53 per cent. of the admissions. In the epidemic of 1851-52, it was 66.7 per cent., in that of 1854-6, 71.2 per cent. In 1859-60, it amounted to 81 per cent.; while, in the Report of this hospital, for 1868, the ratio is stated to have been 84 per cent. of the admissions. How, then, is this melancholy result to be accounted for? To the failure of the vaccine lymph, or to some other cause? It may, no doubt, be referred to the fact, that the causes productive of small-pox have been in operation to a greater extent of late than previously; or, at the commencement of the present, and the latter half of the past century. Mr. Wagstaffe, who published some observations, in 1722, on the prevalence of small-pox in children, states, that the mortality did not exceed *one* per cent. of the cases. Isaac Massey, apothecary to Christ's Hospital, in a pamphlet published the following year, remarks, "that not one in fifty had died of small-pox in that institution during the previous 20 years; and only *one* during the previous nine years, although some hundreds have been down with it." Jenner, also, speaks of a mild form of small-pox, which existed in his day; so mild, in fact, that, in hundreds of cases, there was no death, while it was not regarded by the common people with any fear. But, during the late visitation of small-pox, the ratio of mortality of the unvaccinated cases amounted to 44 per cent., and in the Stockwell Asylum, in 1871-2, to 60 per cent. We can thus understand, why the ratio of attacks, with the vaccinated, has been gradually on the increase of late years. The causes productive of small-pox being more powerful now, the prophylactic virtue of the vaccine matter has been rendered less efficacious than before. Nor does vaccination prevent returns of the disease, the outbreaks of small-pox having increased, *pari passu*, with the increase of vaccination. There was no visitation of small-pox, in London, from 1796 to 1825; but there have been ten outbreaks, during the last 37 years; viz., in 1840-1; 1844-5; 1847-8; 1851-2; 1854-5; 1859-60; 1862-4; 1866-7; 1870-71; and 1876-7. All that vaccination does, is to mitigate the severity of the attack: although even this advantage is diminishing, the rate of mortality, *with vaccinated cases*, being greater now than formerly. For the 20 years —1836 to 1855—the rate of mortality of the vaccinated cases, in the small-pox hospital, was 6.56 per cent.; in 1863-4, it amounted to 9.2 per cent.; and at the Stockwell Asylum, in 1871-2, it was 11.8 per cent. Should this progressive rate continue, the time may come, when there will be little or no difference betwen the vaccinated and the un-vaccinated.

There is another fact, that is particularly worthy of consideration, and which is in direct opposition to the doctrine of contagion. This is the occurrence of sporadic cases. Although the disease only appears epidemically at particular periods, there are few, if any years, in which there are not isolated, or sporadic, cases. But the disease never spreads under these circumstances, although the attacks are quite as severe as when it prevails epidemically. How, then, is this anomaly to be explained? It can only be accounted for in one way; this is, by concluding, that small-pox is not infectious—at least, not by the emanations from the sick. The only way in which it can be propagated, as I believe, is by the direct introduction of the morbid matter into the blood, by inoculation. Even then, the effects are much less, and very different, in general, to those observed in the natural disease.

Being thus unable to account for either the origin or the spread of small-pox, by the *contagion* theory, it becomes an interesting subject of inquiry to ascertain, what the real cause of its production is. Observing that the majority of fevers—all specific, or essential, fevers—are the product of one particular agent; and finding that small-pox has been the invariable accompaniment of the plague and other epidemics, the inference, that this disease is produced by the same cause, cannot be a very improbable or a very illogical one. Assuming, then, for the sake of argument, and in the absence of all other assignable causes, that small-pox is a product of malaria, it remains to ascertain, how this agent could be productive of a disease, that differs so much, apparently, from all other fevers. The problem may not be quite so difficult of solution as it would at first sight appear.

We have already seen, that different effects are produced, under different circumstances, by the operation of malaria on the human body: and hence the various forms of ordinary, or endemic, fevers—a result that has been ascribed to the greater or less quantity of poison present in the system; and the greater or less rapidity with which it is introduced. There can be no reason, therefore, why another effect should not be produced by a combination or variation of the same circumstances: although this effect—viz. the pustular eruption—is entirely different to what is observed with ordinary fevers. Other eruptions, however, are common, particularly in Typhus and Typhoid fevers, while, in the latter, there is an eruption on the internal surface of the intestinal canal, which assimilates greatly to that of the pustular eruption of small-pox. Dr. William Budd remarks: "This disease of the intestinal follicles is, in fact, a true *exanthema* of the bowel. In some cases, indeed, so salient are all these points of analogy; and so striking is the family likeness between the cutaneous eruption (of small-pox) and the intestinal disease; that the conclusion, just stated, involuntarily starts to the mind on the first view of the morbid appearances." In proof of this, the Writer states, that "a student, who had charge of the examination (of a body) asked, in all simplicity, whether the case was not one of small-pox, which had fallen on the bowels instead of on the skin." (*Op. cit.*, p. 48.) As there can be little doubt, after the arguments and facts previously adduced, that typhoid fever is a product of malaria, it can be no stretch of the imagination to conclude, that small-pox is also an effect of the same almost universal cause. The only difficulty is to ascertain, if it can be ascertained, how this particular effect is produced, as all conclusions on the subject must be to a great extent, if not altogether hypothetical. The effect may be due merely to an increase in the quantity of the poison introduced into the system: so that greater effects, such as cuticular inflammation, etc., are produced than under other circumstances. Similar results are constantly observed in the severe forms of continued fever—internal inflammation and abscesses being the common *sequelæ*, or accompaniments of these affections. On the other hand, the phenomenon may be caused by the way in which the morbid matter is introduced into the system: or, the greater or less rapidity of its introduction. It is to this circumstance, that we must ascribe the production of typhus at one time, typhoid fever at another.

When the poison is introduced in a certain quantity, and with a certain rapidity, into the arteries, the system takes the alarm: spasm is produced, and the poison is retained in the venous or large capillaries, as in typhus and other forms of fever. But, when the morbid matter is introduced slowly, or in lesser quantity, it is enabled to penetrate the minute capillaries; hence the inflammation and the pustular eruption of the small intestines and of the skin, observed in Typhoid fever and in small-pox.

To the last-named effects—the inflammation and pustular eruption—may, probably, be ascribed the circumstance, that small-pox and typhoid fever only attack a person, with some few exceptions, once in a life-time. "The disease, named small-pox," says Dr. Budd, "only occurs once in a life, simply because the small-pox *cannot grow again*, in a body in which it has once bred." (*Op. cit.*, p. 35.) That is no doubt the fact; but why can it not grow again? This is a question, that neither Dr. Budd nor any one else has either answered, or attempted to answer. Other fevers grow again, or, to use a more appropriate term, prevail again and again, in the same person: then why should not typhoid fever and small-pox? The reason is probably this. If it be necessary, as before inferred, for the poison to enter a particular set of vessels, in order to produce the pustules, we can hardly fail to infer, that the inflammation and the suppurative process would obliterate their cavity. The same effects, therefore, could not be observed again, excepting in those rare cases in which the obliteration was not complete, or in which the whole of these particular vessels were not implicated in the inflammatory process.

It may be asked, if typhoid fever and small-pox be produced by the same cause, how does it happen that the same poison is productive of a pustular eruption on the intestinal canal at one time, and on the skin at another? Although it is impossible to offer a satisfactory explanation of this phenomenon, the result, it may be remarked, is not more singular than that observed with nearly all poisons; and, we may add, with the majority of remedies, which produce different effects according to the dose. Tartar emetic, for instance, manifests its influence almost entirely on the skin, or the stomach, or the intestines, according to the quantity taken; as does turpentine on the capillary system, or the kidneys, or the bowels, under the same circumstances. There is nothing, therefore, to prevent small-pox from being a product of malaria, notwithstanding the variation observed between this disease and typhoid fever, or between small-pox and other malarious fevers. It is not necessary to have a specific poison for each specific disease: on the contrary, were such the case, it would be an anomaly, and in direct opposition to all the laws of Nature, as well as to the axiom laid down by Newton: A

MULTIPLICITY OF EFFECTS, BUT A PAUCITY OF CAUSES. Leaving this part of the subject, which is, of course, entirely speculative at present; and which I should not have entered into but for one reason—a reason that will presently appear—we will now pass on to a consideration of the treatment of this terrible and dreaded disease.

"TREATMENT."—Having long since concluded, that small-pox belongs to the class of malarious fevers, I have been in the habit, for a great many years, of administering the different forms of carbon in this disease. Not that I expected to cure such a disease, or to remove the eruption, but merely with a view of lessening the fever, mitigating the symptoms, and preventing the supervention of severe and malignant effects. For this purpose, it was my object to employ the remedy at an early period of the attack: but, as this was not always possible, the antidote was also given at later periods, and in all stages of the disease; with what success will presently appear. Having, for the reasons already stated, lost the histories of the cases thus treated, I can now only give the general result.

When the antidote, or carbonic acid gas, has been given at the commencement of the attack, and on the first appearance of the eruption, the fever has been cut short, the pustules rendered more circumscribed, and the suppurative process apparently lessened. In short, the attack has invariably been a mild and uncomplicated one. When, however, the pustular eruption had been fully formed, the employment of the remedy did not appear, and could hardly be expected, to exercise much influence on the suppurative process. It removed the fever, however, prevented the case from becoming confluent, and rendered the supervention of malignant symptoms impossible. That carbonic acid gas had this effect, may be concluded from the result that has been obtained by the administration of this agent in malignant and confluent cases, as shown in the following histories; which, fortunately for the present inquiry, have been recorded by other Practitioners, who had made use of the same remedy as myself. The first case was attended by Dr. Dobson.

Ann Forbes, who was suffering from an attack of confluent small-pox, had been subjected to an antiphlogistic mode of treatment, during the first, or inflammatory, stage of the disease. "But the secondary, or putrid, stage having commenced—the patient presenting one mass of *putrid ichor*, with sickness, trembling, a small and rapid pulse, and great restlessness—effervescing draughts were had recourse to. In 24 hours, the putrid stench was much diminished: the breath of the patient was not near so offensive: in two days, she was still better; and, by repeating the purgative, giving wine occasionally, and persevering in the use of 'fixed air,' her recovery was surprisingly speedy and perfect." (*Op. cit.*, p. 36.)

Dr. Hayworth, of Chester, who was induced to employ the same remedy, communicated to Dr. Dobson the result of two cases of small-pox so treated. The first was that of "Master Davies, a year and a half old, who, on the 8th day, had 20 black pustules on the body. Thirty-six hours after the commencement of the treatment—or the administration of, what Dr. Haygarth calls, the *aqua mephitica alkalina*—no more black or gangrenous spots appeared; and, on the 16th, the sloughs fell off, leaving so many ulcers in their room. Some of these were two inches in circumference. Under the continued use of the gas—an ounce being taken every four hours—the ulcers gradually healed, and the little patient was restored to health." In the next case—a bad confluent one —in which the gas was administered, Dr. Haygarth remarks: "I cannot so far *flatter myself* as to think that it will prevent the second fever; but I assure you, much beyond my hopes, this patient had scarcely any signs of it." Another case is this:

> "Mr. Sandbach, who had witnessed the good effects of 'fixed air,' in the case of Master Davis, was induced to administer it to a patient (æt. 19) suffering from an attack of confluent small-pox. It was the 11th day of the disease, and the patient's face was covered, almost entirely, with a dark-coloured scab: the pustules, on many parts of the body, were of a livid hue; a number on the arms, legs, and body, had run together, and formed large bladders, some of the size of a crown piece, filled with a dark coloured serum. The pulse was extremely quick and small; the tongue dry and black: great restlessness, subsultus tendinum, and some degree of delirium. Under these circumstances, I had little to expect from medicine of any kind; I, however, ordered him the *aqua mephitica alkalina*, and acid julep—the patient having taken no medicine previously. On the following day, the fever was much abated; the patient less delirious, and every symptom more favourable.

From this time he gradually grew better, and continued the medicine for 10 or 12 days, but the convalescence was slow." (Dobson, p. 44.)

I have, on several occasions, asked medical Friends, who had charge of small-pox patients, in public Institutions, to allow me to treat a certain number of them: but the reply has been, that they could not transfer their charge to another, and an unauthorized person. I then asked them to give the remedy a trial themselves. Whether they did so or not, I am unable to say: if so, they must either have been disappointed in the result; or, else, did not take the trouble to furnish me with the details. So, also, on the outbreak of small-pox in the Island of Jamaica, in 1852, shortly after the subsidence of the epidemic cholera, and subsequently to my return to England, I wrote a letter, which was inserted in the *Colonial Standard* (June 24th, 1852), recommending the employment of carbon and its compounds, and, more particularly, the gaseous form of it. A Friend—Mr. Trench of Lucea—wrote me afterwards to say, that my suggestions had been adopted, in his part of the island, and that he himself had been treated on my plan, to which circumstance he attributed his recovery. But I have never been furnished with any *medical* evidence on the subject.

In addition to carbonic acid gas, I have generally administered one or more of the hydro-carbons at the same time, and for reasons that have been already explained. As the duration of this disease is longer than with the majority of fevers, and as the principal part of the poison is concentrated on the external surface, considerable benefit ought to be derived from the employment of these agents, in the arrest and removal of the pustular eruption.

Lastly, it is only necessary to add, that the same arguments and the same conclusions will apply to Scarlatina and Measles, which are, as I infer, produced by the same cause, and are amenable to the same mode of Treatment.

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